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PHENOMENAL REGRESSION TO THE 'REAL' OBJECT. II.

BY ROBERT H. THOULESS.

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I. PHENOMENAL REGRESSION UNDER SIMPLIFIED CONDITIONS OF PERCEPTION.

(*a*) *The problem.*

IN an earlier part of this paper (1), it was suggested that 'memory colours,' the tendency to constancy of size, and a similar tendency of perceived shapes, could be regarded as examples of a general tendency in perception for phenomenal characters to be intermediate between the characters indicated by retinal stimulation and the 'real' characters of the perceived object.[1] For this tendency, the term "phenomenal regression to the 'real' object" was suggested.

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[1] The sense in which the term 'real' is here being used is sufficiently clear from the characters which are called the 'real' characters. These are the characters of continuous extension, constant brightness (reflectivity), constant colour (selective reflectivity), etc., attributed to the object of common experience. By the 'real' object is meant the object of commonsense, not the metaphysically real object or noumenon, and not the real object of the physicist, a discontinuous collection of molecules which are not perceived and whose properties are not attributed to the object in ordinary experience.

It is self-evident that such regression could not take place if the subject had no awareness of the 'real' character supplied either by his previous knowledge of the object or by his present perception of it. If, in reporting on the apparent sizes of two discs of different real size, he neither knew beforehand nor could now see which was the larger, then obviously their relative physical sizes could not affect their relative phenomenal sizes.

There remain, however, the possibilities that what determines the influence of the 'real' on the phenomenal character may be previous knowledge of the real character, or that it may be present perceptual indications of the real character, or that it may be a combination of both of these factors. The term 'memory colours' strongly suggests that the explanation of phenomenal regression in brightness or colour is to be found in previous experience. The subject remembers that the white paper is really white and therefore tends to see it as the brighter even when the luminosity of the other is the greater. This, however, is clearly not the only possible explanation of the effect. The subject also sees the more reflective paper as less strongly illuminated—*i.e.* as farther from the light or as shadowed. His tendency to see this paper as the brighter may be a response to these perceptual indications of its less strong illumination. The problem which of these factors is really the determining cause of the effect or whether both contribute to it must be decided by experiment.

(*b*) *Experiments on brightness.*

That present perceptual indication of relative illumination rather than memory is the explanation is suggested by Hering's own observation (2) that when the two surfaces are examined through a blackened tube which cuts out all surrounding objects the less luminous white surface is seen as the less bright. Plainly what has been eliminated here is the present perceptual indications of the relative illuminations of the papers, while memory of their relative reflectivities has remained unchanged. This non-quantitative experiment, however, proves only that present perceptual indications are an important factor in the production of the effect. It remains possible that memory of the 'whiteness' of the one paper and the 'greyness' of the other may also be a factor, because this observation proves only that with monocular observation through a tube, the relative phenomenal brightness of the papers is changed (presumably becoming closer to their relative luminosities): it does not prove that the relative phenomenal brightness is then equal to the relative luminosities.

To prove whether or not the effect disappears with monocular observation through a tube, it is necessary to perform a quantitative experiment. In my earlier paper (1), I described an experiment with papers of known reflectivity under measured differences of illumination, and it was shown that the position at which a subject reported that they appeared equally bright when looked at with one eye through a tube was also the position at which the calculated luminosities of the two papers were equal. I have repeated this experiment with different papers and with many other subjects, and have found always the same result. There is no phenomenal regression in comparing the brightnesses of adjacent surfaces with one eye through a tube, although phenomenal regression is present and may be considerable in amount when the tube is removed and the surfaces are seen separated and with both eyes.

This experiment proves that the tendency to constancy of brightness depends only on present perceptual indications of the relative illuminations of the two objects and not at all on previous knowledge of their relative reflectivities, for it is these perceptual indications alone which have been altered by looking at the papers through a tube, while previous knowledge or memory of the relative 'whiteness' or reflectivity of the two papers has remained unaltered. The term 'memory colours' is, therefore, inappropriate.

(c) *Experiments on shape.*

It is less easy to eliminate the effect of phenomenal regression on the perception of shapes and sizes. Monocular observation of an inclined circle produces a reduction of the axis-ratio of the phenomenal ellipse, but it does not make the phenomenal ellipse equal to the perspective ellipse (as it would be if phenomenal regression were altogether eliminated), and, in some cases, the reduction is very small. The subject S., for example, observing with two eyes a circular disc at such an angle that the axis-ratio of the perspective figure was 0·26, matched it with an ellipse of axis-ratio 0·575. With one eye, he matched it with an ellipse of 0·55. The index of regression was lowered only from 0·59 to 0·555. In Table I, A and B are the results with two subjects of experiments on phenomenal shapes and sizes with binocular and with monocular observation. Observation of shapes through a tube led to no further appreciable reduction of the effect beyond that produced by simple monocular observation. Binocular observation through a pseudoscope also failed to eliminate the effect completely, although it reduced it considerably further, as is shown in Table I C.

Table I. *Reduction of phenomenal regression by monocular observation and by observation through a pseudoscope.*

(A) Experiment: ellipse with long axis away from subject giving a circular phenomenal shape.

Subject	Method of observation	Ratio of vertical to horizontal axis			Index of regression
		Real	Phenomenal	Stimulus	
S.	Binocular	1·35 : 1	1 : 1	0·35 : 1	0·78
	Monocular	1·43 : 1	1 : 1	0·37 : 1	0·735
M.	Binocular	1·74 : 1	1 : 1	0·62 : 1	0·465
	Monocular	2·36 : 1	1 : 1	0·84 : 1	0·17

(B) Experiment: circular discs of different real sizes at distances making phenomenal sizes equal.

Subject	Method of observation	Relative size of larger disc			Index of regression
		Real	Phenomenal	Stimulus	
S.	Binocular	1·335 : 1	1 : 1	0·635 : 1	0·61
	Monocular	1·335 : 1	1 : 1	0·701 : 1	0·545
M.	Binocular	1·335 : 1	1 : 1	0·661 : 1	0·59
	Monocular	1·335 : 1	1 : 1	0·774 : 1	0·47

(C) Experiment: (top) determination of phenomenal shape of ellipse with long axis from subject arranged to give stimulus shape approximately circular, and (bottom) determination of phenomenal shape of inclined circle.

Subject	Method of observation	Ratio of vertical to horizontal axis			Index of regression
		Real	Phenomenal	Stimulus	
S.	Direct binocular	3·93 : 1	2·67 : 1	1·05 : 1	0·70
	Through pseudoscope	3·93 : 1	1·60 : 1	1·05 : 1	0·32
S.	Direct binocular	1 : 1	0·50 : 1	0·2675 : 1	0·47
	Through pseudoscope	1 : 1	0·375 : 1	0·2675 : 1	0·31

These results show that phenomenal regression in the perception of shapes and sizes is, at least in large part, determined by the actual presence of cues indicating the real shapes and sizes of the objects observed. When these are partially eliminated the effect is reduced. The failure to make it disappear may mean that the mere memory of the real shape or size of the object also plays a part in producing phenomenal regression. It may, however, mean simply that cues to the real shape or size have not been sufficiently eliminated. We have undoubtedly eliminated the indications of the real character of the object which arise from binocular perception of distance relationships, but various cues as to the relative positions of the far and near edges of the disc in the shape experiment and of the two discs in the size experiment still remain.

The subject can still see, for example, the relationship of the object or objects to the surface of the table, and it is known that this is enough to indicate distance relationships both in monocular vision and in vision through a pseudoscope.

A more adequate experiment to determine whether previous knowledge was a factor in the production of phenomenal regression was carried out by means of the experiment on the perception of shapes. It was necessary to devise a method of experimenting in which all cues to distance relationships of different parts of the disc were absent in monocular vision. This was done by making the subject observe the discs through a window in a framework covered with black velvet, inside which the discs were presented at various angles on a turntable with a horizontal axis of rotation. All parts of the apparatus were blackened, and the light inside the chamber was so reduced that while the white disc was clearly visible nothing else could be seen. In order to be able to eliminate the factor of previous knowledge three discs were prepared, one circular, one elliptical with axis-ratio 0·7, and the third elliptical with axis-ratio 0·5. These were presented to the subject at such angles that the perspective figure was an ellipse of axis-ratio 0·7, 0·5, or 0·3. Thus a perspective figure of ratio 0·5 might be given by the circle at an inclination of 30°, by the 0·7 ellipse at an inclination of 45° 30′, or by the 0·5 ellipse viewed normally.

Preliminary experiments established the fact that monocular indications of the actual shape of the disc were successfully eliminated: the subject was not able to distinguish between the circle at 30° and the 0·5 ellipse viewed normally when he observed with one eye only, although he could see the difference with two eyes.

The experiment was carried out under three conditions: (I) the subject observed with two eyes and was also told which of the three discs was used, (II) the subject looked with one eye but was told and shown which disc was put in, and (III) the subject used one eye and did not know which disc he was observing. In each method of experimenting, the three discs were presented in irregular order. The purpose of the two elliptical discs was solely to make no. III a condition in which the subject had no knowledge of the physical shape of the object seen. Results obtained from these discs were not therefore used in the following table (Table II, page 7), which contains only the results from the circular disc. Each is the mean of eight observations.

It will be seen that, whereas with binocular observation there is always considerable regression to the actual physical shape of the disc, there is not a significant difference between the perspective and phenomenal shapes for monocular vision whether or not the subject has been informed of the actual shape of the disc shown. This result is shown diagrammatically in Fig. 1, which represents the values of the axis-ratios for the phenomenal ellipse corresponding to the three values of the stimulus axis-ratio for the three methods of presentation. The continuous line inclined at 45° to the base shows the curve which would be obtained if there were no phenomenal regression. The lines joining the values for methods II and III approximate closely to this line, while the values for method I stand well above it, showing the effect of phenomenal regression.

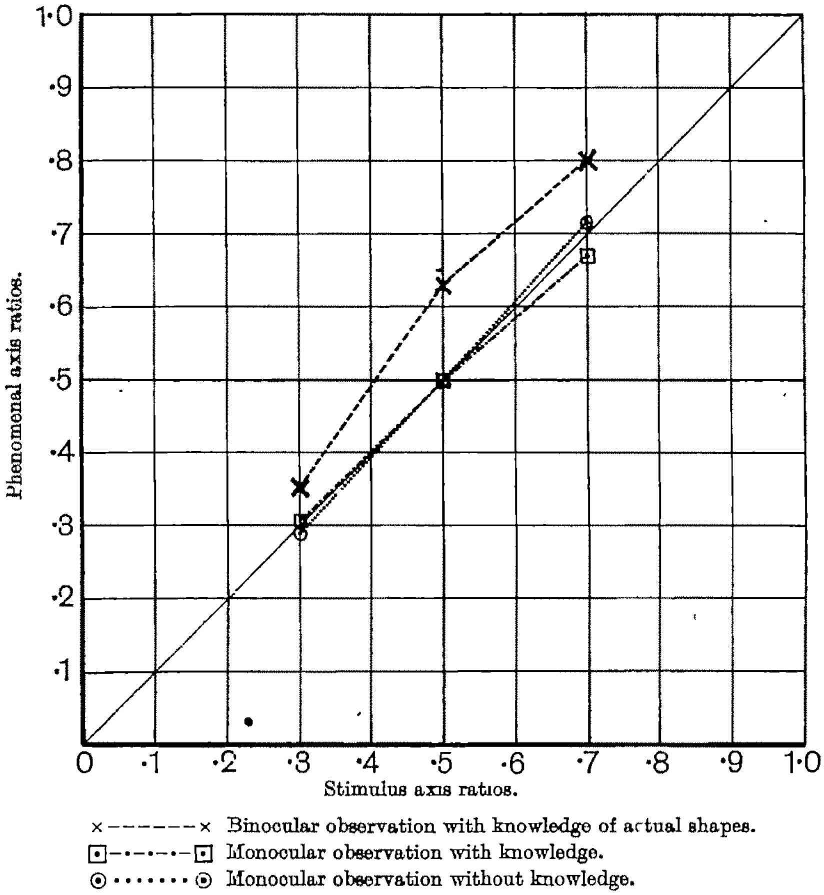

Fig. 1.

(*d*) *Conclusions.*

The tendency to constancy of shapes, therefore, like the tendency to constancy of brightness, is not at all dependent on the subject's previous knowledge of the actual shape of the object looked at, but only on the presence of indications given in perception of what this shape is. These indications are the perceptions of the relative positions of the near and far edges in the shapes experiment, the relative distances of the large and small discs in the size experiment, and the relative distances from the light or the degree of shadowing in the brightness experiment. Let us call these 'perceptual cues' to the 'real' character of the object. The law of phenomenal regression is that: *when a stimulus which by itself would give rise to a certain phenomenal character is presented together with perceptual cues which indicate a 'real' character of the object, the resulting phenomenal character is neither that indicated by the stimulus alone nor that indicated by these perceptual cues, but is a compromise between them.*

The fact of the dependence of phenomenal regression on the presence of perceptual cues to the real character of the object observed makes it certain that the numerical amount of the regression obtained will depend on the whole of the experimental conditions, and that measurements made under different experimental conditions will not be comparable. For example, experiments on phenomenal shapes reported in the first paper were carried out by means of discs lying on a table. The surface, the edges, and the end of this table were all visible to the subject, and all provided cues to the real shape of the disc in addition to that provided by the binocular perception of the relative distances of the near and far edges of the disc. In the results shown in Table II, all such additional cues were eliminated. It is, therefore, not surprising that in the latter

Table II. *Comparison of amounts of phenomenal regression when a circular disc is observed*; (I) *with both eyes*; (II) *with one eye with knowledge of actual shape of disc*; (III) *with one eye but without this knowledge.*

		Method I		Method II		Method III	
Angle of inclination of disc to the line of vision	Perspective shape	Phenomenal shape	Index of phenomenal regression	Phenomenal shape	Index of phenomenal regression	Phenomenal shape	Index of phenomenal regression
44° 30′	·70	·80 ± ·008	·375 ± ·027	·67 ± ·009	− ·12 ± ·039	·72 ± ·010	0·08 ± ·04
30°	·50	·63 ± 020	·26 ± 03	·50 ± ·012	0·0 ± ·03	·50 ± ·008	0·0 ± ·02
17° 30′	·30	35 ± ·009	·13 ± ·022	·305 ± ·007	·015 ± ·02	·29 ± ·01	− ·03 ± ·03

experiments even with binocular vision (method I), a lower index of regression was obtained than that of the same subject in the earlier experiments. He was reacting to less adequate cues to the real shape of the object.

For the same reason my experiments on the tendency to constancy of brightness are not quantitatively comparable with those of Katz (3) and Köhler (4). My cue to the differences in illumination of the two papers was provided by the subject's perception of their relative distances from the source of light, while both Katz and Köhler reduced the illumination of their more reflective paper by throwing a shadow on it. The cues, in this case, must have been provided in some such way as by the perception of the shadow cast on the surrounding objects. It is clearly impossible to know beforehand whether these cues would be more or less effective than those provided by the distances of the paper from the lamp, and it is also clear that the effectiveness of such cues must depend on the exact nature of the surrounding objects on which the shadow is cast, the sharpness of the outline of the shadow, and so on. Any comparison of the degree of phenomenal regression in animals and man or any investigation of individual differences between human subjects must be carried out by means of experiments in which all surrounding conditions are exactly equivalent in all respects.

It is not only by closing one eye, cutting out the perception of surrounding objects, etc., that phenomenal regression may be diminished, but by any method which reduces the effectiveness of the perceptual cues to the real character of the object. In my experiments, these perceptual cues were mainly cues to relative distances and there are certain manners of observing which, by making distance perception more difficult, tend to destroy the character as three-dimensional object of what is perceived, reducing it to a two-dimensional patch or system of patches. Such devices are, for example, partially closing the eyes so that the sharpness of outline is destroyed, throwing the object out of focus, or steadily fixating one point on it. All of these devices reduce the amount of phenomenal regression without getting rid of it altogether. It is necessary to warn subjects specifically against such methods of observation, particularly against partial closure of the eyes, since this is taught as a method in drawing.

II. SUCCESSIVE AND SIMULTANEOUS INDUCTION OF BRIGHTNESS.

Let a subject fixate a point between two discs of different shades of grey, the darker of which is so much more strongly illuminated than the other that they appear equally bright. As has already been shown, this will be when the darker grey is so much the more strongly illuminated that its luminosity (*i.e.* its stimulus intensity) is greater than that of the other. If, after fixation, the subject obtains an after-sensation of the two discs on a uniform screen, these are not equally bright—the less luminous paper producing the brighter after-sensation. If, on the other hand, the luminosities of the papers are made equal, they produce after-sensations of equal intensity, although the more reflective paper appears the brighter at the time of fixation.

This result can also be obtained with the shape and size experiments. If an ellipse with long axis away from the subject is arranged so that it appears to him to be circular, the after-sensation obtained on a screen at right angles to the line of vision is that of an ellipse with long axis horizontal, while a truly circular after-sensation is obtained from such an ellipse casting a circular image on the retina, although this appears to the subject as an elliptical shape with long axis vertical. Similarly, two circular discs of different sizes adjusted to phenomenal equality give unequal after-sensations, whereas when adjusted to stimulus equality they give equal after-sensations, although the objects appear unequal at the time of fixation. This last experiment is a particularly striking one, since the subject is astonished at the contrast between the relative phenomenal sizes during fixation and in the after-sensation. It can conveniently be performed with one eye closed, because the amount of phenomenal regression in the size experiment is considerable even with monocular observation.

Phenomenal regression, therefore, while modifying the characters of perception, appears to have no effect on successive induction. The after-sensation is determined by the character of the physiological process in the sense organ itself, while the neural correlate of phenomenal regression must be non-peripheral. After-sensation, being free from the effect of phenomenal regression, may be used as an indicator of the true character of peripheral physiological processes in the sense organ.

Katz (3) made similar observations when the objects showing phenomenal regression found a background for simultaneous induction effects. If equally grey papers are placed before a shadowed disc of a bright shade of grey and an unshadowed one of a darker shade of grey, then, if the two discs are adjusted to equal luminosities, the patches of paper in front of them appear equally bright although the discs behind them appear unequal in brightness. If, on the other hand, the two discs are made to appear equally bright, the patches of grey paper in front of them look unequal in brightness, that in front of the disc of greater luminosity (*i.e.* greater stimulus brightness) appearing the darker. Simultaneous contrast appears, therefore, also to depend altogether on the stimulus character of the background and not at all on its phenomenal character when this differs from the stimulus character.

III. PHENOMENAL REGRESSION TO 'REAL' HUE.

Hering(2) showed that if a blue paper was illuminated by light of an unsaturated yellow colour, it was possible to arrange the relationship of the selective reflectivity of the paper and the colour of the illuminating light so that the light reflected to the eye from the paper was colourless (as shown by monocular examination of it through a blackened tube), but that, nevertheless, the paper looked blue to ordinary binocular observation. This experiment can most conveniently be done by rotating on a colour wheel a variable sector of blue paper with the remainder of the disc white, and illuminating it by such an impure yellow light as that of a carbon filament electric lamp.

The explanation is the same as that for the other regression effects. The disc is 'really' blue; as stimulus it is grey. The cue to its 'real' blueness is given by the perception of the fact that it is illuminated by yellow light. When this cue is eliminated by observation through the tube, the disc is seen in its stimulus colour grey. When, however, the disc is observed with both eyes, this cue to its 'real' colour is perceived, and the phenomenal colour is a compromise between the 'real' blue and the stimulus grey, *i.e.* it is a blue less saturated than that which would have been produced by white illumination.

Similarly a 'really' white object under such coloured lighting appears neither truly white nor so strongly coloured as it would if phenomenal regression to the 'real' colour were not present, but it is somewhat tinged with the illuminating colour. If such a white object is examined through a tube, phenomenal regression is eliminated and the apparent colour is much enhanced.

IV. THE TENDENCY TO ELIMINATION OF SIMULTANEOUS INDUCTION OF COLOUR THROUGH PHENOMENAL REGRESSION TO REAL HUE.

If a grey paper is placed on a coloured background of about the same brightness, it is seen to be tinged with the complementary hue. Hering has shown that this is due to a complementary physiological process on the part of the retina stimulated by the grey paper. Amongst other consequences of the physiological reality of the induced process is the fact that this part of the retina gives an after-sensation roughly complementary to the perceived hue (*i.e.* roughly equivalent to the original background colour). In other words, the grey paper on, let us say, a green background is physiologically equivalent as stimulus to a reddish paper without that background. This being the case, it is remarkable that striking simultaneous contrast effects in colours are so difficult to obtain. If the grey is of different brightness from the green or if it is separated from it by a too definite dividing line, the appearance of reddening may be small or (to most observers) unnoticeable. If, however, a grey paper on a green background is fixated under such conditions that there is no apparent reddening of the surface, it nevertheless produces a green after-sensation, showing that the peripheral physiological 'red' process has taken place as before[1]. In accordance with the principle of phenomenal regression, the paper which is 'really' grey has been seen as grey, although it is equivalent as physiological stimulus to a reddish paper.

The rôle of phenomenal regression in reducing colour contrast effects as phenomena when they are present as peripheral physiological facts may be made clearer by a quantitative experiment.

A circular disc of green paper (*L* of Zimmermann's colour scale) was mounted on cardboard. In this disc was cut an aperture in the shape of a cross, immediately behind which a Marbe colour wheel was rotated. On the colour wheel was mounted a paper of the same hue as the disc itself, together with a neutral grey of the same brightness. It was possible, therefore, to fill the cruciform aperture in the disc with a series of colours of unchanging brightness ranging from a neutral grey to a green of the same hue as the background. In all cases the subject was required to report the apparent hue of the cross to inspection and also to fixate one of the inner corners of the cross for 30 seconds, and to report the colour of the after-sensation of the cross obtained on subsequently looking at a sheet of neutral grey.

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[1] The use of such terms as 'red' process and 'green' process does not imply the acceptance of Hering's theory of unitary complementary processes. Both processes may involve more than one of three primary physiological processes in colour vision. Whatever theory of colour vision we adopt, it is necessary to admit the physiological reality of the process of simultaneous colour induction.

When the cross was of pure grey, all subjects saw it as of a reddish hue, since conditions were favourable to the appearance of colour contrast. By putting in an increasing amount of green, this reddish hue was diminished and finally disappeared. The cross was seen as grey although the after-sensation was still strongly green, showing that the red physiological process had taken place on this part of the retina. When the amount of green was further increased, the cross was seen as green, although the greenness of the after-sensation still showed that the red process had predominated in this part of the retina. Only by a very much greater increase of the green in the cross did the after-sensation become itself reddish, showing that the green process had predominated in the retinal image of the cross. Results of this experiment for four subjects are shown in Table III.

Table III. *Effect of phenomenal regression in reducing phenomenal simultaneous colour induction.*

Subject	Mean colour composition of figure for elimination of induced colour in immediate experience (*i.e.* point of elimination of reddish colour in cross)	Mean colour composition of figure for elimination of physiological complementary process (*i.e.* point of elimination of green in after-sensation of cross)	Proportion of peripheral retinal process eliminated by phenomenal regression (%)
R.P.	257° green, 103° grey	311° green, 49° grey	17·5
O.P.	118° green, 242° grey	184° green, 176° grey	36
S.	64° green, 296° grey	235° green, 125° grey	73
K.F.	42° green, 318° grey	243·5° green, 116·5° grey	83

Between the stages, therefore, at which the figure contains so little green that it is seen as reddish on a green background, and that at which it contains so much green that the retinal process is itself the green process in spite of the background, there is a stage at which the red process takes place on the retina but the figure is seen as grey. There is also a longer stage in which, while the retinal process is the red process, yet the figure is seen as green. These facts are exactly analogous to the other effects of phenomenal regression. When the figure is seen as grey the object is 'really' green, while the peripheral physiological stimulus is the complementary red process. The phenomenon is a compromise between these, and is grey. When the green in the object is further increased it is seen as green, although still the red process is taking place on the retina. The compromise effect of phenomenal regression is still taking place, but the peripheral physiological red stimulus has been decreased while the 'real' green has been increased, so that the phenomenon is green although a less intense green than that of the real object.

It appears then that colour induction effects take place as physiological facts far more frequently than they occur as phenomena. The effect of phenomenal regression is to make simultaneous induction disappear partly or almost entirely from immediate experience even when it is present as peripheral physiological process. This explanation is confirmed by the observation that the conditions in which phenomenal colour induction is diminished are also those in which phenomenal regression is most effective in other experiments. If a sharply defined outline surrounds an object on a coloured background, simultaneous induction tends to disappear; if, on the other hand, the outline is confused (as by putting thin tissue paper over the object) simultaneous induction is enhanced. In the same way, if the outline of an inclined circle is blurred by partially closing the eyes, it is seen as equivalent to an ellipse nearer to the perspective ellipse. In each case the effect of phenomenal regression has been diminished by blurring the outline. The possession of a sharp outline favours the organization of the perception as that of an object segregated as a separate whole from its background. Failure of such organization reduces the effect of the 'real' character of the object on perception. Köhler (5) has pointed out that such segregation of the object is also much reduced if the brightness is the same as that of the background even though the hue is different. It follows, therefore, that equality of brightness should favour the appearance of simultaneous colour induction by reducing phenomenal regression. It is, of course, well known that there is such heightening of phenomenal colour induction when the brightness of object and background are equal.

The effect of blurring the outline in the above experiment was investigated with the subject K.F. When the cross was covered with tissue paper, the reddish coloration persisted until the composition of the figure was 182° green, 178° grey. Since the point of change of colour of the after-sensation was 243·5° green, 116·5° grey, it follows that with the tissue paper only 25 per cent. of the physiological process of red on the retina was eliminated from immediate experience by phenomenal regression, whereas 83 per cent. was the proportion eliminated when the cross was observed in the ordinary way.

Another method by which phenomenal colour contrast can be reduced has been pointed out by Mr J. Tennant (6). If two *V*-shaped pieces of the same grey paper are placed on contiguous fields of complementary colour, induced colours are seen strongly on the grey papers. If, however, the grey papers are joined together by a band or are made continuous with a background of the same grey, the induced colours disappear as phenomena partially or altogether. Again phenomenal regression has been increased by the introduction of conditions which favour the seeing of the grey in its 'real' colour. The making of the grey into a continuous field favours the organization of it as a unitary phenomenon, and, therefore (by Fuchs's principle of assimilation (7)), as of a uniform colour.

It is well known that individuals differ in the extent to which they see induced colours. Some habitually see colours in shadows whereas others only see them under the favourable conditions which can be produced in a laboratory experiment. Table III suggests that there are two factors in this individual difference, one physiological and the other psychological. The physiological factor is the extent to which the complementary process takes place in the retina. R.P. needed 311° green in the figure to eliminate the complementary retinal process, whereas C.P. needed only 184° green. This physiological factor is, however, in these four cases, less variable than the psychological factor—the degree to which phenomenal regression to the 'real' colour takes place. I have tried to measure this in column four by taking the angle of green at which the after-sensation became of neutral hue as a measure of the total amount of physiological red process when the figure was composed of the grey paper alone, and the difference between this and the angle at which the figure appeared grey as a measure of the extent to which phenomenal regression was eliminating the effect of this red process on perception.

The subject R.P., for example, reported that he commonly saw colours in shadows. It appears that this was due both to the fact that the complementary colour process occurred on his retina to an abnormally great extent and also to the fact that there was very little tendency for this complementary process to be counteracted by a tendency to see things in accordance with their 'real' characters. The other experiments also showed this subject to have a low tendency to phenomenal regression.

V. RELATED CHARACTERS OF PERCEPTION.

(a) *The phenomenal upright.*

The direction in space which is perceived by us as upright and into which we tend to make reflex adjustments of the axes of our bodies is determined by two sets of indications derived from different systems of receptors. These are (i) the direction of the downward forces acting on the body and particularly on the mechanism of the inner ear, and (ii) the visual indications provided by the systems of horizontal and vertical planes and lines by which we are surrounded. Ordinarily both of these indicate the same 'upright' direction. Sometimes, however, we are in situations in which their indications conflict. In the cabin of a sailing ship, sharply heeled over, for example, the downward forces remain unchanged in direction, while the system of planes and lines of the floor and walls of the cabin, ordinarily vertical and horizontal, indicate an upright at a considerable angle to this direction. On the other hand, the visual indications derived from the horizontal plane of the earth's surface or from vertical surrounding objects remain unchanged to an observer in the cockpit of a spirally descending aeroplane or in a merry-go-round, while a horizontal centrifugal component due to the circular movement alters the direction of the downward forces acting on the balance mechanisms[1]. In both of these cases there is an alteration of the direction that appears upright and to which the axis of the body is adjusted. The new upright line is intermediate between the direction indicated by the forces acting on the body and that indicated by vision. Thus, on an inclined ship, a lamp kept in the gravitational upright by gimbals appears to be leaning over in the opposite direction to the ship; a person, moreover, standing in the cabin adjusts his body to his phenomenal upright and therefore staggers in the direction towards which the ship is inclined.

This effect is closely analogous to the one we have named 'phenomenal regression.' Here also there are two sets of perceptual cues, either of which acting alone would produce a different perception from the other. When they act together, the perception produced is of a character intermediate between that which would follow from either set of cues acting separately. The difference between the two cases is that we have before been dealing with perceptions in which one cue is commonly felt to be in the privileged position of being the one which 'ought' to determine our perception (*i.e.* it is the actual physiological stimulus) while the other system of cues (indicating the physical character of the object) can be treated as a disturbing factor[1]. Here, on the other hand, we are dealing with a conflict between two sets of sensory cues neither of which has necessarily such a privileged position. Either the visual upright or the gravitational upright or neither[2] may be, in fact, the line normal to the earth's surface.

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[1] I have given other examples of this in the revised edition of Stout's *Groundwork of Psychology* (8).

(*b*) *The law of phenomenal compromise.*

To cover such cases we must state a more general principle: that *when two sets of sensory or perceptual cues which alone would give rise to phenomenal characters inconsistent with one another are presented together, the phenomenal character which is actually experienced is neither that indicated by one nor that indicated by the other set of cues but is a compromise between them.*

Let us call this the *law of phenomenal compromise*, reserving the term *phenomenal regression* for that particular class of compromise effects in which one set of cues indicates the character of the peripheral stimulus while the other indicates the 'real' character of the object perceived. The more general case of phenomenal compromise is that in which the distinction between the two sets of cues cannot be described in these terms—usually because neither indicates a 'real' character of the object. An example has already been suggested above: the phenomenal upright of a subject enclosed in an inclined cabin acted upon by a centripetal force.

Another example was described in Part I of the present paper. If a wall with parallel top and bottom receding from the eyes is observed through a telescope, the top and bottom appear to diverge from the observer. Regression has taken place from the stimulus character of convergence but beyond the 'real' character of parallelism. The character towards which regression has taken place is the divergence which would be the real character of an object which, in the apparent position of the wall as observed through a telescope, would cast on the retina an image with the degree of convergence of the image actually formed.

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[1] As psychologists, of course, we may and should question the assumption that our perceptions 'ought' to be determined solely by the conditions of peripheral stimulation. No one who has carried out such experiments as these, however, will doubt that this assumption is fundamental in the psychological theory of the man in the street. The majority of my subjects were convinced that they ought to have seen the inclined circle in its perspective shape, and that, in not seeing it in that shape, they had made a mistake.

[2] This last condition would be fulfilled, for example, for a subject confined in an inclined enclosure such as an aeroplane cabin, which was also subjected to a centripetal force due to a circular movement through the air. Although difficult to experiment with in this form, the result may be deduced from observations when each of these changes takes place separately. The apparent upright, which is also the line of automatic adjustment of the body's axis, would lie between the uprights of visual and of gravitational indications, although neither of these was the line normal to the earth's surface.

(c) *The size-weight effect.*

I would further suggest the possibility that the psychological explanation of the well-known size-weight illusion is exactly parallel to this. The subject is asked to report on the phenomenal equality of the weights of objects of different sizes. His primary indication of the objects' weights is derived from the intensity of skin pressure and of resistance in the lifting muscles. The phenomenal weight is not, however, determined by these alone for of two objects of equal weight, the smaller (and therefore the more dense) appears to be of greater weight. The character disturbing the perception of weight appears, therefore, to be the objects' relative density. Apparent weight is a compromise between real weight (indicated by muscular and tactual stimuli) and density (the cues to which must be derived both from the combined action of the cues to weight and of visual indications of size).

If this is the correct explanation of the size-weight illusion, we can treat the effect quantitatively in much the same way as was used for phenomenal regression results. Thus the subject S., using Stoelting's apparatus for the size-weight illusion, judged the small standard of diameter 2·15 cm. and weight 55·5 gm. as of equal weight to the variable at weight 68·6 gm. and diameter 3·65 cm. The large standard of weight 55·8 gm. and diameter 8·2 cm. was judged equal to the variable at 31·8 gm. and diameter 3·65 cm. All the weights were of equal height. The results are treated in the following table in a manner exactly analogous to the treatment of the results of the brightness experiment in Table V of Part I of this paper. The densities and weights are expressed as ratios of the lighter to the heavier object. Column 5 contains a numerical index of the extent of the influence of density indications on the phenomenal weight calculated by a formula similar to that used for indicating the amount of phenomenal regression in Part I.

It will be noticed that the index obtained is somewhat less than that for regression to 'real' brightness and much less than that for regression to 'real' size and shape. Density affects our experiences of weight less than do real shape, size and brightness our experiences of apparent shape, size and brightness. The larger amount of regression to the 'real' physical character from the stimulus character is to be expected from the high importance for behaviour reactions of the physical object.

There may also be a further reason for the smallness of the index of phenomenal compromise in the size-weight experiment. If size instead of density were taken as a cue to weight, phenomenal compromise should take place in the opposite direction; the larger objects should appear heavier, not lighter, than they really are. If, therefore, the explanation of the illusion in terms of phenomenal compromise is correct, we can only accept it as an empirically ascertained fact that the effect of density is predominant over size as a cue determining phenomenal weight. The possibility remains that both size and density exert their influence in determining weight experiences, but in opposite directions, the influence of density predominating. Unfortunately this is not a hypothesis that can be decided directly by experiment, since of the three factors (weight, size and density) only two can be independently varied. The only indication that one might hope to have from experiment would be if, experimenting with a wide range of different densities and sizes, it were found that there are critical values of these beyond which the illusion is reversed in direction.

Table IV. *Size-weight illusion expressed as example of phenomenal compromise.*

(1) Objects compared	(2) Ratio of their physical weights (S_2)	(3) Ratio of their phenomenal weights (P)	(4) Ratio of their densities (S_1)	(5) Index of phenomenal compromise from weight to density $\frac{\log P - \log S_2}{\log S_1 - \log S_2}$
$\frac{\text{Small standard}}{\text{Variable}}$	0·81	1	2·34	0·20
$\frac{\text{Variable}}{\text{Large standard}}$	0·57	1	2·875	0·35

VI. PHENOMENAL REGRESSION AND FECHNER'S DIFFERENCE LAW.

The experiments described in this and the earlier paper were first undertaken in the hope that they would throw light on the well-known deviation of the stimulus differences giving equal sense distances from the value required by Fechner's difference law[1]. This law, in its simplest form (that connecting three stimulus values), is that if $a - b = b - c$, then $A/B = B/C$ (or $B = \sqrt{AC}$)—if a–b represents the sense distance corresponding to the presentation of the stimulus A with B, and $b - c$ the sense distance similarly resulting from the stimuli B and C. In other words, the middle stimulus value must be the geometric mean of the two extreme stimulus values for the production of two equal sensory steps.

The experimental investigation of the truth of this law has shown, however, that the intermediate stimulus B is not the geometric mean of A and C, but lies between their geometric and arithmetic means, although different experimenters have differed as to its nearness to the one or the other.

Merkel found that B lay much nearer to the arithmetic than the geometric mean, while other experimenters (such as Delboeuf, Lehmann, Angell and Neiglick) found the intermediate stimulus closer to the geometric mean of the extreme stimuli (9). It is a very striking fact that all of these experimenters alike have produced results which show B as lying between the arithmetic and geometric means, although some regard the fundamental law as one of approximation to the geometric mean, others as one of approximation to the arithmetic mean.

Wundt boldly put forward the view that there were two laws which he called 'Weber's law' and 'Merkel's law' respectively which were predominantly obeyed under different conditions of experimentation (10). Clearly under all conditions both laws must be regarded as having some effect, but differences of experimental conditions may make one or the other law predominate.

How then are we to explain the tendency of the intermediate stimulus to approximate to the arithmetic mean which is the tendency expressed by Merkel's law? Recent physiological research has made the matter much more difficult for us than it was for Wundt. It is no longer possible to believe, as did Wundt, that the physiological concomitant of a stimulus is directly proportional to the stimulus intensity, and that the change from direct proportionality to logarithmic proportionality takes place between the physiological and the psychological response. Adrian has measured the frequency of electrical discharge in a sensory nerve for different intensities of stimulation, and plots curves roughly logarithmic in form (11). The response to the stimulus of the neuron itself, therefore, tends to the logarithmic form. If there were a further tendency to logarithmic proportionality of the sensational response to its physiological component, then the intermediate stimulus for equal sense distances should be smaller than the geometric mean (*i.e.* farther from the arithmetic mean than is the geometric mean, not intermediate between them).

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[1] Sometimes called 'Weber's law.' Although this usage has high authority it has led to deplorable confusion and should be avoided. The term 'Weber's law' is best restricted to the law connecting differential thresholds with stimulus values.

To account for the results which have been obtained, therefore, we must suppose that in the passage from intensity of neuron activity to intensity of sensational response, there is some tendency which results in the sensational response being more closely proportional to the physical stimulus than is the neuron response. Where can such a tendency be found? In the days when the so-called 'psychological explanation' of Fechner's difference law was still possible, psychologists suggested many reasons why sensation intensity should tend to be proportional to the logarithm of the intensity of its physiological process. Adrian's work has forced us to find a reason why sensation intensity should differ in the opposite direction from its physiological process. Such a reason is to be found in phenomenal regression, the tendency of which is to make what is experienced follow in part the character of the physical cause of the stimulus.

This explanation is strictly analogous to the explanation of the tendency to constancy of size. We saw that the experienced relative sizes of two discs of different actual sizes was not the same as the relative sizes of their retinal images (the physiological stimulus relationship) but was intermediate between this and their relative physical sizes. If we substitute in this statement 'intensity' for 'size,' we have a statement of the combined action of Merkel's and Fechner's tendencies which experiment has found. The experienced relative intensities of two or more lights or sounds is not the same as their relative physiological intensities (which would mean that they obeyed approximately Fechner's difference law), but is intermediate between this and their relative physical intensities (their relative physical intensities alone leading to Merkel's law).

If this is the right explanation, then the degree to which the intermediate stimulus for equal sense steps departs from the value demanded by Fechner's law and approaches that demanded by Merkel's law is a measure of the degree of phenomenal regression and this amount can be indicated as before, by the calculation of an index of phenomenal regression. Thus we can contrast a few specimen results of Merkel (12) and of Neiglick (13), both using light stimuli, the former by a method of successive presentation, the latter by simultaneous presentation of the three stimuli (Table V).

Table V. *Specimen results of investigations on equal sense distances.*

Experimenter	R_1	R_3	R_2	Arith. mean	Geom. mean	Index of P.R.
Merkel	0·5	32	8·3	16·25	4·0	0·52
"	0·5	16	5·45	8·25	2·83	0·61
"	24	1536	472·3	780	192	0·64
Neiglick	1·0	15·14	4·28	8·07	3·89	0·13
"	2·11	15·14	5·78	8·625	5·65	0·05
"	15·14	68·0	31·43	41·57	32·1	−0·07

There is a great difference between the absolute values of the amount of phenomenal regression obtained by these two investigators. Many of Merkel's results showed higher indices than those here calculated. Some of Neiglick's give a negative index (like the last one in the above table), but generally they are small and positive. This difference may well be due to the difference in manner of presentation; successive presentation may favour a regression to the relative physical intensities of the stimuli. Another possible explanation is suggested by an investigation of individual differences in phenomenal regression[1]. It appears that different individuals vary enormously in the amount to which they exhibit phenomenal regression, although the effect is present to some extent in everyone. Amongst nearly a hundred subjects the index of phenomenal regression for the size experiment ranged from 0·11 to 0·95. It may well have happened that the subjects tested by these two investigators differed in the extent to which they exhibited phenomenal regression.

For the above explanation of the discrepancy between Merkel's results and those demanded by Fechner's difference law, it is necessary to suppose that it is possible for our perceptual reactions to be determined by the physical scale of intensities. If a subject had only experienced light intensities of varying strength without any indications of their relative physical intensities, his experience would have provided him with no basis for a tendency to reaction to the physical intensities. Regression from the stimulus intensities could not, therefore, take place unless we assume an innate tendency to react to the physical intensity. Actually the subject has such a basis in experience, although a much smaller one that that, let us say, for determining the physical sizes of objects at different distances. Every time that he has experienced the difference in illumination when one electric bulb or candle has been

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[1] Not yet published.

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replaced by two or by three he has been given the experience necessary to build up a scale of physical intensities. We may notice also, as a possible explanation of the conflict of Merkel's and Neiglick's results, that this experience has in general come from successive and not from simultaneous illumination.

The above explanation is based on the assumption that the logarithmic relationship between sensation and stimulus intensity is at least approximately correct. This is generally accepted, but it may not be the case. Since performing these experiments I have come in contact with an important investigation by Dr Houstoun (14), in which he brings forward strong evidence that the true form of the law relating threshold with absolute stimulus value is that $I/\Delta I$ plotted against $\log I$ gives a normal curve. This would give by a transformation parallel to Fechner's

$$\text{Sens.} = k \int_{-\infty}^{x} e^{-\frac{(\log I - \log I')^2}{2\sigma^2}} \, d(\log I)$$

as the law corresponding to Fechner's law. This would give a curve for S plotted against I which would differ from the logarithmic curve although approximating to its form for part of its course. If Houstoun's law proves to be correct, it will remain a question for future research whether it explains the ascertained values for stimuli giving equal sense distances or whether there is still a residual tendency for the sensation to approximate more closely to simple proportionality with the stimulus than is demanded by the law. The tendency in Merkel's results is so large that it is certain that, whether we accept Fechner's or Houstoun's law, a strong tendency remains for the sense difference to be proportional to the stimulus distance, which tendency may be explained by the principle of phenomenal regression. For the results obtained by other investigators using the method of simultaneous presentation, we cannot be certain whether or not they obey Houstoun's law exactly without further investigation.

VII. THEORETICAL ASPECTS OF THE PRINCIPLE OF PHENOMENAL REGRESSION.

Let us suppose that a subject is asked to report on the shape of an inclined plane figure whose shape is not previously known to him. He might be answering any one of three questions: (1) as to the 'real' shape of the object observed, (2) as to the shape of its plane projection, (3) as to its apparent or 'phenomenal' shape. It is only in answering number 3 that he is performing the simple operation of reporting immediate experience. To give anything approaching a correct answer to the other two questions, he must make allowance for the angle of inclination. In other words, the more complex process of judgment enters in. Since, moreover, the data for making a correct judgment are inadequate in the important respect that the subject does not know the degree of departure of the phenomenal from the stimulus shape (and commonly supposes it to be zero) his judgments are not accurate.

In fact, it is the answer to the third question that the average subject gives when asked how the object 'looks' from the point of view from which he is observing it. To make sure that he is answering this question, I have generally given him preliminary practice with an inclined ellipse before doing the experiment with the inclined circle, pointing out at the end of the preliminary experiments that I want to know neither what the shape of the object really is nor how he thinks it ought to look but simply the shape it does look to him. Even the most ignorant subject understands these instructions perfectly and it is only rarely and with exceptionally sophisticated subjects that one finds that they try to adopt special devices for judging the stimulus character of the object observed.

The average subject supposes that he sees the stimulus character. After demonstration of the effects of phenomenal regression, he is still convinced that he ought to see the stimulus character and believes that the experiments have demonstrated that he has made a mistake. Often he thinks that if they were repeated he would not make the same mistake again. In fact, all that demonstration of the effect gives him is an essential piece of knowledge for making a reasonably correct guess at the stimulus character, but if he obeys instructions and reports his immediate experience instead of trying to guess the stimulus character, demonstration of the effect does not appear to alter the phenomenon.

I took, for example, two entirely naïve subjects who had heard nothing of the nature of my experiments or of their results. They were tested in the shape experiment with a circle inclined at such an angle as to give a stimulus figure of axis-ratio 0·36, and in the size experiment with a standard circle of 39·7 cm. diameter at 230 cm. from the eyes with a variable of 29·7 cm. diameter. The nature of the effect was then demonstrated completely to them. They held out an ellipse of ratio 0·35 at right angles to the line of vision and saw that it approximately covered the inclined circle. They also measured the projected diameters of the circular discs in the size experiment at the position of phenomenal equality and found them unequal and were then shown the two circles at the position of stimulus equality, again confirming by measurement. They were then retested. The original results and the results of the re-tests are shown in Table VI. Each measurement is the mean of a complete ascent and descent by the limiting method. It will be seen that the demonstration has had no appreciable effect on the amount of phenomenal regression.

Table VI. *Records of two subjects in experiments on phenomenal regression in shape and in size before and after explanation and demonstration of the effect.*

A. Shapes experiment.

	Ratio of major to minor axis			
	In 'real' shape	In 'stimulus' shape	In phenomenal shape	
Subject			Before demonstration	After demonstration
J. L.	1 : 1	1 : ·36	1 : ·525	1 : ·525
			1 : ·575	1 : ·55
			1 : ·575	1 : ·575
J. B.	1 : 1	1 : ·36	1 : ·625	1 : ·60
			1 : ·60	
			1 : ·6125	

B. Size experiment.

	Ratio of diameter of large to that of small circle			
Subject	'Real'	Phenomenal	Stimulus (when distance of small circle is such that the phenomenal sizes are equal)	
			Before demonstration	After demonstration
J. L.	1·34 : 1	1 : 1	1 : ·72	1 : ·745
			1 : ·73	
			1 : ·755	
			1 : ·745	
J. B.	1·34 : 1	1 : 1	1 : ·49	1 : ·475
			1 : ·51	1 : ·51
			1 : ·51	
			1 : ·51	

It is sometimes stated that the tendency to constancy of size can be destroyed by adopting an analytical attitude. I have entirely failed to confirm this statement. If the two discs in the size experiment are arranged at such distances that their plane projections are equal, and the subject is asked to confirm this by measurement, he still sees the 'really' smaller as the phenomenally smaller, so long as he looks at them with both eyes fully open and correctly focused. I have not found any subject who could make them appear equal by a change of mental attitude.

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Practice without information as to results, also, has no constant tendency to reduce phenomenal regression. Many subjects show a change in the amount of phenomenal regression in the first two or three experiments, but this is as often in the direction of increasing as of decreasing tendency to regression. This tendency to initial change makes it necessary to give each subject a few practice experiments until he shows an approximately constant effect.

The popular view that the perceived phenomenon must (unless influenced by error) reproduce the characters of peripheral stimulation rests on a naïve summative psychology of perception. To this view, each different part of a complex stimulus produces its own sensation, and the perception is regarded as a complex whole built up as a sum of those parts. A more adequate psychological theory emphasizes rather the interaction of the elementary sensations and the essential newness of the resultant experience and speaks of the 'integration' of the constituent sensations into a perception. A more radical step is made in the same direction when the phenomenon resulting from complex stimulation is regarded as a new response not necessarily containing in itself either changed or unchanged the simpler phenomenal responses which would result from separate stimulation by the parts into which the complex stimulus can be divided. This view of perception, a characteristic doctrine of the Gestalt psychologists, is being increasingly held by many who would not regard themselves as members of that school. All such views of perception may be distinguished from summative or integrative theories by being called 'response' theories of perception.

Let us suppose that the stimulus a produces the sensation (or the simple perception) A and that the stimulus b produces the sensation B. What will be the experience produced by the simultaneous action of a and b? To the summative theory the experience produced will be $A + B$. In this extreme form, this theory has probably never been consistently held although it is often assumed (particularly by physiologists and physicists when their work leads them to deal with psychological matters). To the integrative theory, the experience produced will be $\overline{AB}$, a genuinely new experience not necessarily containing A and B as introspectively recognizable parts but nevertheless derived from them. To a response theory of perception, the stimulus $a + b$ is a different stimulus from either a or b, and may well provoke a totally different perceptual response C which may be neither composed of A and B nor derived from them.

The experimental results on phenomenal regression seem best explained on a response theory. On a summative or integrative theory they remain 'illusions' (*i.e.* atypical facts of perception which require exceptional explanations). On a response theory, the ellipse seen with binocular observation of an inclined circle is a fact of perception of the same order as the ellipse seen with monocular vision and elimination of distance cues. The former differs from the latter in the greater complexity of its stimulus; not at all in its character of shape as given in immediate experience. One is the shape of the plane projection of the object while the other is not; neither is the shape of the actual object. The fatter ellipse of binocular observation does not contain the thinner perspective ellipse as part of it.

The summative view which would regard the phenomenal ellipse as containing the perspective ellipse as part of itself is false to the facts. The integrative view which regards the phenomenal ellipse as produced from the perspective ellipse by a process of integration on the psychological level is condemned by the principle of economy of hypothesis. The perspective ellipse is a fact of peripheral physiological process; there is no ground for asserting any corresponding mental fact. Integration may be admitted to take place at the physiological level. The most economical psychological hypothesis is to regard the phenomenon as the psychical correlate of the end result of the process of physiological integration.

The facts of phenomenal regression show clearly the inadequacy of that view of our knowledge of the external world which regards the prime data of experience as mere sense impressions or their combination in 'perspectives,' from which the intuition of things and of their real characters is reached by a process of inference. The immediate data of experience (the phenomena) are not pure sense-impressions but are already tainted with the characters of reality[1]. Köhler has shown that phenomenal regression takes place in hens and chimpanzees (4); Frank has demonstrated it in children of eleven months (15). While there is evidence that the influence of the real character increases somewhat in later years[2], it seems improbable that there is any stage of human development at which the phenomenon is free from this influence of reality.

The essential fact of phenomenal regression is that an important factor in the determination of perceptual response is the nature of the physical object so far as the conditions of perception are such that the observing subject has any cues as to what are the characters of the physical object. Since our perceptual responses are primarily parts of the process of adapting our behaviour to outside objects, this effect of the physical nature of the object is not surprising. Physical objects and physical characters are of predominant importance for behaviour adaptations. Characters of stimulation without importance as indications of real character tend to be lost altogether to experience. We do not, for example, experience to a noticeable extent the great and rapid decrease of physiological process which takes place during the first few seconds of the action of a light of uniform intensity on the retina.

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[1] If we use the term 'reality' in the sense described earlier (p. 1, n.).

[2] (15) and some results (not yet published) of my own.

More surprising than the dominant effect of the physical object is the fact that this effect takes place by the way of compromise. Stimulation of a part of the retina in an elliptical patch of axis-ratio 0·7, together with perceptual cues to the fact that the physical cause is a circular disc, produces (in the subject *S.*) neither the experience of seeing an ellipse of ratio 0·7 nor that of seeing a true circle, nor any tendency to oscillate between these two experiences. It produces instead a single phenomenal shape identical with that produced by an ellipse of axis-ratio 0·8 viewed normally. This is neither a correct indication of the shape of retinal stimulation nor of that of the real object but is a compromise between them.

VIII. PHENOMENAL REGRESSION AND THE LAWS OF PERSPECTIVE.

The child learning to draw in accordance with the laws of perspective is being taught to draw a projection of the object he is looking at on the plane of the picture. In order to discover what are the shapes, sizes, etc., he is required to draw, he is taught to use the device of holding his pencil at arm's length and, with one eye closed, to make measurements along it. The same result has been obtained by the use of such an optical device as the *camera lucida.*

That the laws of perspective do correctly describe the ways in which shapes, sizes, relationships of lines, etc., must appear on such a plane projection cannot, of course, be denied, and it is obvious that the experiments on phenomenal regression have no bearing on this question. Nor do they throw any doubt on the usefulness of the above devices for determining the characters of the plane projection.

Teachers of perspective are, however, often not content to make these legitimate claims for the laws of perspective. They are inclined to say that the laws of perspective are the laws of the ways in which we 'see,' and to suggest that the above-mentioned methods of measurement with ruler, etc., are devices for finding out how we 'really' see things.

The psychological nature of the process of learning to draw in accordance with the laws of perspective is obscured and the difficulty of the process underestimated by this common supposition that the child is being "taught to draw things as he really sees them." His drawing response is, on the contrary, being reconditioned to the stimulus object instead of to the phenomenal object. I have evidence that this reconditioning alters to some extent the degree of phenomenal regression. It does not abolish it altogether. Experienced artists and teachers of perspective show the effects of phenomenal regression in my experiments.

Some artists, moreover, have departed very far from perspective drawing. I have found that certain of the post-impressionist painters drew inclined objects in ratios which were about those of the phenomenal shapes as measured in the above experiments (12). It seems probable, therefore, that these were actually drawing the phenomenal and not the perspective figure. What is the justification (if any) for this? The prime object of representational drawing is, of course, neither to reproduce shapes and sizes as the artist sees them nor to reproduce them in accordance with the laws of perspective, but to draw them in such a way that the perception of the drawing produces the same perceptual response of shapes and sizes as would the perception of the physical objects. How this is to be attained depends on the degree to which the drawing produces in the observer the impression of a third dimension. If the illusion of depth in the picture were perfect, the perspective shape or size would clearly be the one to be drawn, since the seen shape would undergo phenomenal regression for the observer of the drawing and, if the artist drew the phenomenal figure, this regression would have taken place twice, making the object distorted to the observer. At the opposite extreme to this, we have the picture which attempts only to be a two-dimensional pattern. This was the declared aim of some of the post-impressionist painters. Here clearly the representational aim would be best attained by the drawing of the phenomenal figure. This may explain why such painters as Matisse have drawn the phenomenal figure. Most drawing, however, lies between these two extremes, and the representational aim should be best attained by a drawing which is intermediate between the stimulus figure and the phenomenal figure. Probably most artists who have not consciously aimed at accurate perspective but have painted as looked best to them, have struck some such compromise.

Experiment shows that the extent of phenomenal regression differs very greatly from one individual to another. It follows, therefore, that a drawing which would look right to one person might look very wrong to another. It is possible that those artists whose departures from perspective seem to most observers to produce distortion of shape are simply those with abnormally large indices of regression. It seems to be a common opinion that children drawing such an object as a top of a cup draw it as a true circle from whatever angle it is viewed. Actual measurements of the drawings on which such an opinion is based would probably show, however, that it is simply an ellipse of large axis-ratio. Beyrl's results indicate that the amount of phenomenal regression for children is somewhat less than for adults (16); but my experiments show that most adults who drew the phenomenal shape would draw an ellipse of large axis-ratio.

On the whole, there is probably sufficient ground for justifying the teaching of the strict laws of perspective to those learning to draw. A simple set of rules is better than a complicated one. Moreover, the uncorrected tendency is to draw the phenomenal figure which is at the opposite extreme of error (if the object is realistic representation), and the combined effect of the uncorrected tendency and the learning of strict perspective rules may well result in a practically satisfactory compromise.

The effect of a knowledge of the facts of phenomenal regression on the practice of drawing are as follows: (1) It enables us to understand the psychological basis of the departure from perspective of primitives, post-impressionists, and such artists as the Japanese and Persians. (2) It undermines the psychological argument for regarding perspective drawing as the one right method of drawing, thus justifying the adoption of other conventions by those whose object is not naturalistic representation but design. (3) It shows that the real difficulty in learning to draw in perspective is greater than is supposed by those who think that the laws of perspective are the laws of how we see things.

IX. SUMMARY.

Phenomenal regression is found to depend not on memory of the 'real' character of the object observed but on the actual reception of cues to that character. After-sensations are found not to be influenced by phenomenal regression at the time of fixation but to depend only on the stimulus characters of the objects observed. The tendency of phenomenal regression is shown to be to reduce apparent simultaneous induction effects. Observations on the perceived upright suggest a more general principle of phenomenal compromise of which the 'size-weight' illusion may be an example. Experimental divergences from the difference law in experiments on equal-appearing intervals may also be explained by phenomenal regression. The facts of phenomenal regression appear to favour a 'response' hypothesis of perception. The theory of the teaching of drawing must be modified by recognition of the fact that the laws of perspective are laws describing the methods of plane projection and not laws of the way in which we see things.

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THE PROBLEM OF INTERFERENCE[1].

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I. THE GENERAL MEANING OF 'INTERFERENCE'.

THE notion of interference is used frequently for explanatory purposes, especially in regard to certain phenomena of learning. In view of this it seems desirable to examine the process of interference and to see whether the theory of the subject is capable of greater precision and further development. In the upshot it has seemed desirable to link up the somewhat scattered work on interference with the systematic work of Pavlov and his co-workers on inhibition.

The term interference can be applied to a considerable field of phenomena. It manifests itself as a loss of efficiency, generally, it is true, only temporary. In cases of distraction, for instance, such an activity as the solving of arithmetical problems may be temporarily impaired in efficiency by the noise of a klaxon horn. Efficiency may likewise be diminished by a persistent mood or worry or by some obtrusive bodily condition such as toothache. Again, practising the cancellation of 'd's' in prose material may have a detrimental effect upon subsequent cancellation of 't's' in similar material. Or the learning of certain material may impede the learning and subsequent recall of other material (generative and reproductive inhibition). Further, the interpolation of certain kinds of activity may have a detrimental effect upon recall (retro-active inhibition).

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[1] This paper was read in substance before Section J of the British Association at Bristol, 1930.

Certain reservations have to be made in regard to the explanations of interference proposed in this paper. First, only the simpler cases of interference have been worked out in detail. In the bulk of the non-experimental cases of interference and in many of the experimental cases matters are more complex than in the simple cases here discussed. This should be borne in mind when considering the more complex cases of interference, for although it is believed that the explanation here proposed is adequate to deal with them, yet its concrete application is by no means always obvious, and in some cases could probably be worked out only by experimental means. Secondly, it is not necessary, so far as this paper is concerned, to make any assumptions as to the underlying conditions of the evocation of a given response by a given situation, *i.e.* to take up any definite position in regard to the nature of association. Thirdly, when situations *A* and *A′* are referred to, it is not necessary to decide whether the kinship between *A* and *A′* is based on partial identity of contents or on similarity. For convenience the term similarity, or an equivalent, is generally used, but this is without prejudice to the unsolved problem of the nature of similarity. Finally, the linking up of Pavlov's external and internal inhibitions with interference no more implies any special theoretical views as to the physiological basis of inhibition than such terms as retro-active and reproductive inhibition have been considered to imply them. It is possible to accept and to use the highly important phenomena that Pavlov and his school have discovered and described, and even to use their terminology in referring to the phenomena, without assenting to their interpretations of those phenomena.

II. TWO GROUPS OF 'INTERFERENCE.'

Cases of interference fall into two groups. In the one group it is assumed that a response *B* can be elicited by situation *A*. The situation is now varied, but the same response *B* is still required. Any delay, failure or impairment of the response *B* due to variation of the situation is to be ascribed to interference. In the other group of cases it is again assumed that the occurrence of situation *A* elicits response *B*. But when situation *A* is varied from *A* to *A′*, a different response *C* is to be elicited or established. Any failure, delay or difficulty in getting *C* elicited by or established to situation *A′* is again to be ascribed to interference where the difficulty is due to the tendency of the situation *A′* to elicit response *B* instead of response *C*. In both groups of cases *A′* may vary all the way from complete identity or similarity with situation *A* to complete non-identity or zero similarity. But whilst in the first group of interference cases interference will vary inversely in magnitude with the degree of similarity between situations A and A' in so far as similarity is involved, in the second group of cases interference will vary directly with the degree of similarity between situations A and A'.

III. 'INTERFERENCE' AND 'EXTERNAL INHIBITION.'

The first class of interference cases can be elucidated in the light of Pavlov's findings. He showed that 'extra stimuli,' *i.e.* modifications of the eliciting situation A, exercise an inhibitory effect upon a response B that is being established to the situation A, when the changed situation evokes a response other than the one required. Thus a positive alimentary response to a flash of light may be inhibited by an investigatory response if the evoking situation is modified by the presentation of an obtrusive sound or smell. And this inhibition will not generally disappear immediately upon the disappearance of the sound or smell but will persist for some time as an inhibitory after-effect. The inhibition exercised by the usurping response upon the required response has been termed by Pavlov an external inhibition[1].

Pavlov further found that in the majority of cases an external inhibition is of a temporary nature even though the 'extra stimulus' persists or recurs. "If they (the extra stimuli) act upon the animal repeatedly and are not reinforced with any unconditioned or conditioned stimulus, sooner or later they become lost upon the organism and lose all their inhibitory properties[2]."

These findings may be applied to such experiments as those by Ford. He found that distractions, such as the noise of a klaxon horn, or of a gramophone, at first increased the performance time for arithmetical problems, but that this detrimental effect rapidly waned, particularly with monotonous distractions[3].

The analogy between such cases and those reported by Pavlov is strong. The initial and temporary impairment of efficiency may be ascribed to external inhibition; the disappearance of this to the neutralization process. Ford himself speaks of a negative adaptation to the distraction. As a result the required response is elicited at normal efficiency, even when the situation is varied by the introduction of a klaxon horn or gramophone noise. There is thus an extension of the range of situations that elicit the required response without external inhibition.

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[1] Pavlov, I. P., *Conditioned Reflexes* (Oxford, 1927), pp. 44–47 and p. 112.

[2] Pavlov, *op. cit.* p. 46. Pavlov does not appear to have described in detail the nature of this neutralization process. It may perhaps be considered as due partly to the establishment of an internal inhibition of the unreinforced interfering response and partly to the generalization of the reinforced response. If this be so, one may see in it a partial explanation of the process of settling down or adjusting to environmental conditions that takes place when even a familiar situation is reinstated. It is significant that Ford found that efficiency declined temporarily on cessation of the distraction.

[3] Ford, A., *Amer. J. Psychol.* XLI (1929), p. 1 ff.

A similar interpretation suggests itself for certain experiments by Shepard and Fogelsonger. In one experiment they trained subjects to respond in a certain way when either of two signals was given. When, however, the two signals were given together, there was not an acceleration of the response, but, on the contrary, delay and hesitation. The subjects reported that the required response was given only when one of the two signals was ignored, *i.e.* when the new situation had been reduced to one of the two original situations. Generally Shepard and Fogelsonger found that the addition of a single foreign element to a situation increased the response times, although the subjects had been forewarned and instructed to make the usual response. It may be supposed that initially the modification of the situation elicited a response other than the one required, the latter being subjected to an external inhibition. Among such interfering responses that of surprise may be mentioned[1].

'Extra stimuli' or modifications of the experimental situation differ greatly from one another in regard to the profundity and range of the inhibition exercised, in regard to the duration of the after effect, and in regard to their resistance to neutralization. Pavlov found cases of 'extra stimuli' which were neutralized either with extreme difficulty or not at all. Such cases do not appear to be rare. "An example may be found in the reaction to acid. If the dog is given food, even some considerable time before the conditioned stimulus to the acid is applied, this reflex cannot be obtained. What happens is that the alimentary centres inhibit the reaction to acid for a considerable time after they themselves are set into activity, and this happens always, no matter how often the experiment may be repeated....Another example may be chosen in the season when the females are in heat. If the males have been housed near the females before the experiment, it is found that all their conditioned reflexes are inhibited in greater or less degree[2]." The marked and persistent external inhibitions exercised in these cases are clearly closely akin to the interference exerted by bodily states such as toothache and indigestion, by emotional states such as moods and excitement, and by stimuli such as tickle and itch. One interesting case is that in which certain social situations induce attitudes of self-consciousness. It is common knowledge that in such cases even the practised responses of speech and walking may be impaired. In these cases, however, neutralization is usually possible.

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[1] Shepard, J. F. and Fogelsonger, H. M., *Psychol. Rev.* xx (1913), p. 290 ff.

[2] Pavlov, *op. cit.* pp. 46–47.

Investigations of interference apart from those carried out by Pavlov and his school have on the whole confined themselves to recording the variations of efficiency that accompany the variations of the situation. The interfering response is, however, clearly of considerable interest and importance. In very many of the recorded cases of interference it is possible to show that the interfering and interfered-with responses are competing for some or all of the same muscle groups. The competition or rivalry may actually take place on the situational side, the response being determined by the particular situational configuration set up. On the other hand, there is some reason to believe that the details of the interference set up, its depth and persistence, are governed to some extent by the relations between the responses involved.

In this connection there is the interesting case in which the situation or part-situation to which a certain response is to be conditioned, is itself the 'unconditioned stimulus' for an unconditioned response. "If we make a tactile stimulation of the skin the stimulus to a conditioned reflex, it frequently happens that we are bothered with an interference from the unconditioned reflex response to the cutaneous stimulus itself, in the form of the scratch reflex or some form of quivering reflex. This may in rare cases be so troublesome that the conditioned reflex never reaches a stable value. Exactly the same thing happens sometimes with musical tones of exceedingly high pitch, it being in some dogs impossible to overcome the difficulty of the resulting sharp motor response[1]." Pavlov speaks of having "observed on many occasions the simultaneous existence of several different reflexes, leading of course to an interaction between them which resulted either in the predominance of one or another reflex or in their mutual neutralization[2]."

This type of case raises the question of the fate of the response to a given situation, whether conditioned or unconditioned, if another response is conditioned to that situation. To a certain extent the problem is discussed in connection with the second kind of interference. Both the evidence there adduced as well as that just given suggest strongly that the situation-response tendency is not destroyed, but that a differentiation takes place of a somewhat labile character so that, owing either to special temporary conditions, or to the relations existing between the responses, interference may still arise. In view of the tendency to explain forgetting as an effect of interference, at least to some extent[1], it would seem that a qualitative study of the responses involved and incident in recall or reinstatement would be a valuable addition to the more usual type of investigation, that confining itself to a quantitative study of loss of recall efficiency.

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[1] Pavlov, *op. cit.* p. 66.

[2] *Ibid.* pp. 65–6.

IV. THE EVOCATION OF NEW RESPONSES: 'INTERNAL INHIBITION.'

Whilst in the first group of interference cases, uniformity of response is aimed at in spite of variation of situation, in the second group there is to be both change of situation and change of response. There is to be not a varying situation calling out the old response *B*, however wide the variation, but a new situation *A′* calling out a new response *C*, however closely (within the limits of discriminability) the new situation *A′* approximates to the old situation *A*. The decisive factor in regard to the direction and elimination of the interference is the incidence of the reinforcement[2]. In the first group the response *B* is always reinforced and no other response reinforced, in the second group *B* is reinforced only when it occurs in response to situation A, response C being reinforced in regard to situation A'.

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[1] Cf. chap. xv (by W. S. Hunter) in *The Foundations of Experimental Psychology*, ed. C. Murchison (1929) on the one hand, and Freud's *Introductory Lectures to Psychoanalysis* on the other.

[2] It should perhaps be pointed out that Pavlov uses the term 'reinforcement' in order to refer to the giving of the unconditioned stimulus and to the consequent evocation of the unconditioned response, subsequent to the initiation of the conditioned stimulus. The type of case of learning with which Pavlov deals, is not perhaps the most representative. It is not generally possible in cases of learning to point out any unconditioned stimulus or response. Nor does the identity of the conditioned with the unconditioned response seem generally applicable, even with the aid of second order conditioned responses. Incidentally it may be noticed that this second feature of Pavlov's cases obviates one of the problems of learning, that of the occurrence of a particular response to the experimental situation. His experiments start at this point, although they throw much reflected light on what happens before this point is reached. In view of this it seems desirable to use the term 'reinforcement' to imply that in certain cases the tendency to respond in a certain way to a given situation is strengthened (reinforced) whilst in others it is not, that whether or not this reinforcement occurs seems to depend upon what happens after the response is initiated and that, in many concrete situations, it is possible to provide the conditions for this reinforcement or non-reinforcement, however difficult it may be to state the process of reinforcement in precise theoretical terms. Reinforcement then refers to the fact but does not contain an explanation of the fact. Any adequate explanation must of course cover the Pavlov case (which at first sight appears to need no explanation), as well as the more complex cases.

The response B has been established to the situation A. Then the situation A' is presented and it is understood that the response C to that situation A' will be reinforced, whilst the response B to that situation will be unreinforced. At first in a number of cases there will be a phase in which owing to the difference between situations A and A' the response B will be inhibited. This is to be explained as due to the external inhibition exerted upon B by the investigatory or other response elicited by the variation of the situation[1].

This phase, which does not always occur, is succeeded or forestalled by one in which the situation A' evokes the response B. This is the phase of generalization[2]. In this phase the new response C will be subject to the external inhibition exerted by the response B. As, however, the response B to situation A' is never reinforced, an internal inhibition will gradually be set up in virtue of which situation A' tends to inhibit B. By means of this internal inhibition it eventually comes to pass that response C is freed from the external inhibition and can be elicited by the situation A'.

The phase of generalization is the phase in which interference is normally met with. It accounts successfully for generative and reproductive inhibition. Retro-active inhibition is similarly accounted for by the generalization of A' when an attempt is once more made to elicit response B by presentation of situation A. Response C tends to be given to A and exerts an external inhibition upon response B[3].

As the interference due to generalization is permanently removed by means of an internal inhibition and as with practice a specific internal inhibition can be set up more and more rapidly and is more restricted in its spread and after effect, it is to be expected that this second kind of interference will generally be a phenomenon of the early stages of learning. It is, however, to be noted that, even after the differentiation has become stable and the initial interference has abated, the interference of generalization can be restored by the action of 'extra stimuli' in temporarily weakening (disinhibiting) the internal inhibition which has made possible the differentiation[1]. Although usually temporary, the interference in such cases of disinhibition is often acute. The reversion to earlier habits of speech and conduct that occur in states of excitement can possibly be accounted for in this way.

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[1] Pavlov, *op. cit.* pp. 72–73 and pp. 118–119.

[2] *Ibid.* p. 113 and pp. 118–119.

[3] Whilst the interference included under the heading of associative inhibition would seem to be adequately accounted for by the external inhibition that is due to generalization, there is the further possibility that this interference is supplemented by the internal inhibition which abates the external inhibition that is due to generalization. As, however, the data does not yet require any such supplementary hypothesis and as Pavlov has not yet dealt with the case where situation A elicits response B and situation A' elicits response C but has expressly reserved this case (*op. cit.* p. 220), confining himself to the case where A elicits and A' inhibits B, it will perhaps be advisable to postpone consideration of this possibility until more data is available.

V. FUNCTIONS OF 'SITUATIONAL SIMILARITY.'

The difficulty of setting up different responses to situations A and A' clearly seems to be a function of the degree of similarity between A and A'. The greater the similarity between the situations the greater the difficulty. This is well illustrated by a case in which a positive alimentary response was conditioned to a luminous circle. Then the animal was made to differentiate from the circle a number of ellipses of equal surface and luminosity. This was done successfully first for a ratio of the semi-axes of 2 : 1, then for ratios approximating more and more to unity till a ratio of 9 : 8 was reached. "The ellipse with ratio of the semi-axes 9 : 8 proved to be the limit at which differentiation just failed. Some indication of differentiation appeared at first, but on repetition it gradually disappeared, and with it disappeared also all the previously established coarser differentiations[2]." On starting again at 2 : 1 and working up once more, the same result recurred. In view of the fact that the differential response to 9 : 8 occurred at first (due of course to an external inhibition), it would appear that in some cases at least the ability permanently to set up different responses to A and A' (a differentiation based on internal inhibition) falls short of the limits of temporary discriminability.

Pavlov points out that success in setting up different responses to A and A' depends upon the method employed. "It has been shown that the development of a differentiation of two very closely allied stimuli may be attempted directly, or, on the other hand, the same differentiation may be effected in stages, leading up through the differentiation of more remote stimuli....If we begin with the first method we generally find that the differentiation does not become established even after a considerable number of contrasts of the two very closely allied stimuli; but if we proceed to establish a differentiation of a remoter stimulus, working up gradually through finer differentiations until the very closely allied stimulus is again reached, it is found that this differentiation is now very rapidly reached[1]." In the bulk of the experiments on interference the less effective (from the point of view of learning) of these two methods has been approximated to. This has no doubt contributed to increase the amount of interference found, but the fact that the differentiations required have on the whole been very coarse, has made it possible to obtain an adequate differentiation by this method. It is important to notice that even coarse differentiations are established only comparatively slowly[2], indicating the considerable range of generalization and consequently of interference, and indicating that the hypothesis is adequate to explain those cases of interference in which the differentiation required is by no means near the limits of discriminability.

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[1] Pavlov, *op. cit.* pp. 115–127.

[2] *Ibid.* p. 133.

There is one type of case in which the generalization that the amount of interference increases with the degree of situational similarity, seems at first to be unsatisfactory. An experiment by Friedman reported by Pavlov will serve as example: "One and the same agent (*e.g.* a tone of 2600 D.V.) served at first as a conditioned alimentary stimulus, later it was transformed into a conditioned stimulus for acid....The following is the general sequence of events: The conditioned stimulus, on transition from one unconditioned reflex to another, very quickly, even within a single experimental day, loses its secretory effect and gives a series of zeros for a comparatively long time; only after considerable practice with the new unconditioned stimulus does it gradually reassume excitatory properties, and these now belong to a conditioned reflex based on the new unconditioned one....After a considerable practice of the conditioned reflexes to acid the conditioned stimuli were once more transformed back again into alimentary ones; the transformation occurred rapidly and only a few reinforcements were needed[3]."

At first sight it might appear that two mutually exclusive responses *B* and *C* have been conditioned to one and the same situation, that here the situations *A* and *A'* are identical. It is, however, more reasonable to argue that the preceding sequence of events, in particular the reinforcement of the one response and the non-reinforcement of the other, must be considered as affecting the situations so as to constitute a real difference between them. *A* and *A'* differ not in regard to the tone of 2600 D.V. but in regard to which of the two responses has been reinforced and which not reinforced. The internal inhibitions thus set up are just as much due to situational differences as those which in human subjects can be effected by means of the instructions given. That the situational differences involved are fairly coarse, is clear from the relative ease with which the transition can be made from the one response to the other. It is important to observe that the transition does not involve a permanent destruction of a situation-response tendency.

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[1] Pavlov, *op. cit.* pp. 121–122.

[2] *Ibid.* p. 122.

[3] *Ibid.* pp. 227–228.

VI. Influence of instructions.

The cases in which in human subjects a transition from one response to another is effected by means of instruction or resolution, is particularly interesting. It has already been pointed out that the phase of generalization is sometimes preceded by a phase in which the external inhibition exerted by the modification of the situation upon the conditioned response produces a temporary differentiation. It would seem that this phase is prolonged in human subjects by the attitude set up by the instructions or resolution. This attitude expresses itself partly in an emphasis upon certain features of the experimental situation and partly as an external inhibition upon the conditioned response. This enables a different response to be made to the external situation. If, however, this attitude is inhibited in any way, *e.g.* by distraction, excitement and the like, the customary response interferes. Speeding the subject up tends to bring the interference into the open as Bergström[1] showed long ago. With human subjects these two phases tend to alternate with one another over a more or less considerable period of learning time, with perhaps a tendency for the maximum interference to occur later than at the outset. The progression from a temporary differentiation based on external inhibition to a stable differentiation based on internal inhibition is complicated by the process of simplification of a situation (a sort of abstraction or analysis) that normally accompanies the stabilization of a situation-response tendency[2].

In the psychological literature there has accumulated a certain body of evidence indicating that interference of the second kind is a function of situational similarity. The perseveration theory, the older and alternative explanation of interference of this second kind, involved originally no inherent relation between interference and similarity. It could perhaps be reconstructed to cover this relation, but there would still be the difficulty of explaining the findings of Robinson and Whitely. They have both shown that loss of efficiency is not maximal when the interfering activity immediately succeeds the activity that on resumption will show a loss of efficiency. Moreover, positive evidence of the importance of situational similarity has been given in experiments of the kind illustrated in the following two examples: If a subject has been trained in sorting cards into labelled boxes arranged in a certain order, he will show greater interference when asked to sort the cards with a new arrangement of the boxes than when asked to sort with new cards and labels as well. Similarly a subject trained in the cancellation of 6's in pages of figures will show greater interference if asked to cancel 2's in pages of figures than if asked to cancel d's in pages of letters. The interference will be greater if the page of figures on which the subject is practised in the cancellation of 6's is the same as that on which he is subsequently asked to cancel 2's, than if a different page of figures is used[1].

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[1] Bergström, J. A., *Amer. J. Psychol.* v (1893), p. 356 ff., vi (1893–1894), p. 433 ff., and p. 267 ff.

[2] "When conditioned reflexes are being established in dogs for the first time, it is found that the whole experimental environment, beginning with the introduction of the animal into the experimental room, acquires at first conditioned properties. This initial reflex could be called, therefore, a conditioned reflex to the environment. But later on, when the special reflex to a single definite and constant stimulus has appeared, all the other elements of the environment gradually lose their special conditioned significance, most probably on account of a gradual development of internal inhibition." Pavlov, *op. cit.* p. 115. This quotation shows one aspect of what is happening. The whole process badly needs experimental elucidation.

Further evidence has been given by Robinson, Skaggs and Whitely[2]. It will be sufficient to describe one of Robinson's experiments by way of illustration. If in the interval between the memorization and recall of a series of eight 4-place numbers, different activities are interpolated, it is found that such interpolated activities as memorizing a series of twenty consonants arranged in a vertical column, memorizing a passage of Scott's *Rokeby*, multiplying 4-place by 4-place numbers, and reading short stories, gave substantially the same recall scores, whilst the interpolation of the memorization of another series of 4-place numbers gave an appreciably smaller recall score.

These findings, pointing as they do to an increase of interference with increase of situational similarity, have been thought by Skaggs[3] and Robinson[4] to raise a difficulty. Each fresh repetition of, say, a series of 4-place numbers can be regarded, they argue, as an activity interpolated between the first repetition and recall. Here the situational similarity is greatest, yet not a decrease but an increase in the recall score results from fresh repetitions of the original series. In order to escape the dilemma they have assumed that as the similarity of the interpolated activity decreases from identity to complete dissimilarity, there will be first a fall in the recall score and then a rise, although never to the original height.

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[1] Newkirk, B. H. and Gundlach, R. H., *J. Educ. Psychol.* xx (1929), p. 291 ff.

[2] Robinson, E. S., *Psychol. Mon.* xxviii (1920), No. 125. Skaggs, E. B., *Psychol. Mon.* xxxiv (1925), No. 161. Whitely, P., *J. Exp. Psychol.* x (1927), p. 489 ff.

[3] Skaggs, E. B., *op. cit.*

[4] Robinson, E. S., *Amer. J. Psychol.* xxxix (1927), p. 297 ff.

In the light of the distinction drawn between the two kinds of interference described in this paper this dilemma would seem to be unreal and the hypothesis eliminating it unnecessary. The dilemma disappears if one considers that in the case of the repetition of the same series with a view to learning it, any interference that arises will be due not to similarity but to lack of similarity in the experimental situations, such as might be caused by variation of the experimental conditions. Here the interference is of the first kind. It is only in regard to the second kind of interference that increase in situational similarity will give rise to an increase in interference. It is clear that the effect of situational similarity differs according as the response is to be modified or to be maintained unchanged.

VII. SUMMARY.

1. It is argued that interference phenomena can be explained in terms of the external inhibition described by Pavlov. The possibility that internal inhibition may also cause interference is left open for the present owing to lack of evidence.

2. In the light of this hypothesis there appear to be two kinds of interference.

(*a*) That due to the variation of the response where a given response is to be maintained in spite of variation of the situation. This kind of interference usually diminishes owing to a neutralization process that is probably based on internal inhibition.

(*b*) That due to generalization or maintenance of the response where it is required to vary the response. Here the interference is severest where the situations to which different responses are to be established, are most alike. The elimination of this kind of interference is based on the setting up of internal inhibitions.

3. The responses themselves and their inter-relationships are important factors in the interference and deserve detailed observation.

(*Manuscript received* 9 *March,* 1931.)

UNSEEN DRAMA AND IMAGERY; SOME EXPERIMENTAL OBSERVATIONS.

BY MADELINE KERR AND T. H. PEAR.

(*From the Psychological Laboratory, University of Manchester.*)

I. *Aims of investigation* (p. 43).
II. *'Radio drama' as a special case of the general problem* (p. 44).
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V. *Results* (pp. 47–53).
  (*a*) *Rating of the vividness of imagery* (pp. 47–49).
  (*b*) *Synaesthesia* (p. 49).
  (*c*) *Emotional reactions* (pp. 49–50).
  (*d*) *Individual partialities* (p. 50).
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  (*g*) *Vividness of auditory imagery and criticism of one's own voice* (p. 52).
  (*h*) *Attitude of listeners towards radio drama* (pp. 52–53).
VI. *Conclusions* (pp. 53–54).

I. AIMS OF INVESTIGATION[1].

THE objects of these experiments were: (*a*) to examine some aspects of the relationship between voice and the expression of personality, the reactions of different individuals to unseen drama, and the function of imagery (of any kind) in the appreciation of unseen drama; (*b*) to investigate auditory imagery, evoked under conditions as natural, and by a situation as concrete, emotion-provoking and 'unschematic' as possible[2].

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[1] The substance of this paper was read to the British Psychological Society on October 25th, 1930.

[2] Bartlett, F. C., *Presidential Address to the British Association* (*Psychology Section*) South Africa, 1929. Pear, T. H., "The relevance of visual imagery to the process of thinking." This *Journal*, 1927, XXVIII, 1–14.

II. 'RADIO DRAMA' AS A SPECIAL CASE OF THE GENERAL PROBLEM.

There have been much controversy and differences of opinion, about the interest, the present and the future of radio drama[1]. It seemed to us that the causes for this might lie deeper than ordinary differences of taste. One might be peculiarities of the listener's predominant imagery. In these experiments an attempt was made to investigate this.

As the plays used were transmitted through a gramophone it would not be fair to call them radio drama, so they will be referred to as 'unseen drama.' The material of both records, however, has been broadcast as radio drama by the British Broadcasting Corporation.

Most previous investigations of imagery have been so far removed from ordinary imaging under non-experimental conditions as to tell us little about the function of imagery in the everyday life of the non-philosophical person. Moreover, interest in the rôle of imagery in abstract thinking has pushed into the background other problems; *e.g.* the relation of imagery to aesthetic appreciation, even to perception. By making the experimental conditions as near as possible to certain everyday experiences (listening to the gramophone, the telephone and the wireless are now everyday experiences of millions of people) it was hoped that the resulting imagery might approximate to that used in ordinary conditions.

III. THE LISTENERS AND THE EXPERIMENTAL CONDITIONS.

Two sets of investigations were conducted with different gramophone records. Record *A* ("Broadcast" record Number X. 6 A, "Making the Christmas Pudding") was by Mabel Constanduros and Michael Hogan of 'wireless' fame, dealing with the misadventures of a Cockney family. Record *B*, "The Safe; a Thriller" (His Master's Voice, Number C 1738) is by Angela Baddeley and company. The first side only of this record was used. Its material is as follows: A woman is working in an office after business hours; her lover, a married man, enters surreptitiously; there is a quarrel; he shuts her in the safe and leaves her to suffocate.

Record *A* was heard by four groups of subjects:

(*a*) Six graduate members of the psychological laboratory.

(*b*) Nineteen students of a first year university class in psychology.

(*c*) Two groups (28 members) of Workers' Educational Association classes in psychology.

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[1] *B.B.C. Year Book, 1931*, London, pp. 184–201. Pear, T. H., *Voice and Personality* London, 1931, pp. 61, 89 f, 95 f, 105 f. "Radio-Drama: Seeing with the Mind's Eye," *Radio Times*, xxxi, No. 392, April 3rd, 1931; "Sincerity at the Microphone," *The Listener*, v, No. 125, June 3rd, 1931, p. 938.

(*d*) Twenty-three university students who had never attended lectures in psychology.

Record *B* was heard by:

(*e*) One group of 112 graduate students taking a post-graduate course in education. The majority had also taken a short course in speech training.

The rooms in which the subjects were seated were made as dark as possible. The laboratory dark room (completely dark) was used for groups (*a*), (*b*) and (*d*). After the record had been played, half a minute was allowed for introspection in silence. Then the subjects were requested to answer a questionnaire, copies of which had been given out beforehand, but at which the subjects had not looked.

The apparatus used for groups (*a*), (*b*), (*d*) and (*e*) was a large Columbia gramophone, with a B.T.H. electrical pick-up and a Marconi moving coil loud speaker, fitted with a baffle board 4 ft. square. Three valves and 130 volts H.T. were required for clear reproduction; 4 volts L.T. were used for the speaker. For group (*c*) a portable Columbia gramophone and a large table gramophone were used.

The subjects were asked to listen to whatever sounds were made in the darkness without any tendency towards 'artistic' or technical criticism. Assurance was given that there was no quantitative interest in the experiment, *i.e.* no marks were to be awarded for the number of items recollected. During the half minute in darkness after the record they were simply to let the memories have their way with them.

IV. A SAMPLE QUESTIONNAIRE.

The following is the questionnaire for record *B*.

Name.
Sex.
Age.
Degree course taken.
Have you had any speech training?
Have you had any musical training?
  (*a*) Instrumental, specify the instrument.
  (*b*) Singing.

Question 1.

Can you call up an auditory image of:
  (*a*) The woman saying "Let me out!"
  (*b*) The woman saying "To-morrow's Sunday; I never thought of that."
  (*c*) Alfred saying "Don't, May[1]."
  (*d*) Alfred saying "Don't know; he didn't see me."

---

[1] Alfred never said this, and the woman's name was not mentioned, but it was thought that this suggestive question might prove useful.

Give each voice a number indicating degree of clearness as on the following key:

1. Perfectly clear and vivid as the actual experience.
2. Very clear and comparable in vividness to the actual experience.
3. Moderately clear and vivid.
4. Not clear or vivid but recognizable.
5. Vague and dim.
6. So vague and dim as to be scarcely discernible.
7. No image at all: you only know in some other way that you are thinking of the object[1].

Question 2.

The voice you have heard is generally agreed to be a good Cockney accent. If you have any auditory image of the voice, is the accent as well marked as when you heard it, or has it drifted towards a normal voice, or towards some other accent? Can you hear the voice now without any Cockney accent at all?

Question 3.

Do you get any imagery which, while seeming to you to be quite important and relevant, might seem to others to be absurd and irrelevant, *e.g.*, do you see any of the voices as colours or feel any of them as having a particular texture?

Question 4.

Do you find yourself reacting emotionally towards either of the voices, or do they appear to you to be artificial and mechanized?

Question 5.

Do you like or dislike the Cockney accent in general? Any special reasons?

Question 6.

Can you visualize:

(*a*) the woman;
(*b*) Alfred?

Question 7.

Are you critical of your own voice:

(*a*) its tone;
(*b*) its accent;
(*c*) its power to influence others?

Question 8.

Do you acquire accents, inflections or phrases easily?

Question 9.

Are you critical of imitations of dialects by your friends or by actors?

Question 10.

Can you visualize these events now:

(*a*) Alfred coming in.
(*b*) Alfred looking at her queerly.
(*c*) Alfred helping her to put the ledgers in the safe.

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[1] This key was adapted from Betts, F. C., "Distributions and Functions of Mental Imagery," *Teachers' College, Columbia Univ. Publication*, 1909.

(*d*) The woman getting in the safe.
(*e*) The woman putting books in the safe.
(*f*) Alfred reaching the books.
(*g*) The safe shutting.
(*h*) The woman knocking.
(*i*) The woman shrieking for Alfred.

Question 11.

Do you see the faces of the various actors if you visualize them?

Question 12.

If you visualize the actors, do you see the whole of their bodies or their faces only?

Question 13.

Are these faces apparently new? Or are they based on actual persons? Or are they a composite of several people?

Question 14.

Was there any irrelevant or discrepant imagery; *i.e.* imagery which seems to you to have nothing to do with the memory of the record?

Question 15.

Do you enjoy radio drama? Any special reasons why you do or do not?

Question 16.

How would you suggest improving radio drama, without adding any visual element, *e.g.* television?

V. RESULTS.

(*a*) *Rating of the vividness of imagery.*

Interesting ratings of auditory imagery were obtained from both questionnaires.

From record *A*, the average scores for each group are:

(*a*)	'Non-psychologists'	3·04
(*b*)	First year psychology students	3·18
(*c*)	W. E. A. students of psychology	3·84
(*d*)	Staff and research students of the laboratory	4·13

$\sigma = 1{\cdot}009$. P.E. $= 0{\cdot}84$.

It thus appears that persons with little or no psychological training tend to rate the vividness of their imagery much higher than those with more psychological training. Betts also found this tendency. F. H. Allport[1] mentions, as a general difficulty of rating, that the more intelligent under-rate and the less intelligent over-rate, their abilities.

If the average score for vividness given by each group of subjects to each of the 5 voices in record *A* be graphed this tendency can be well seen.

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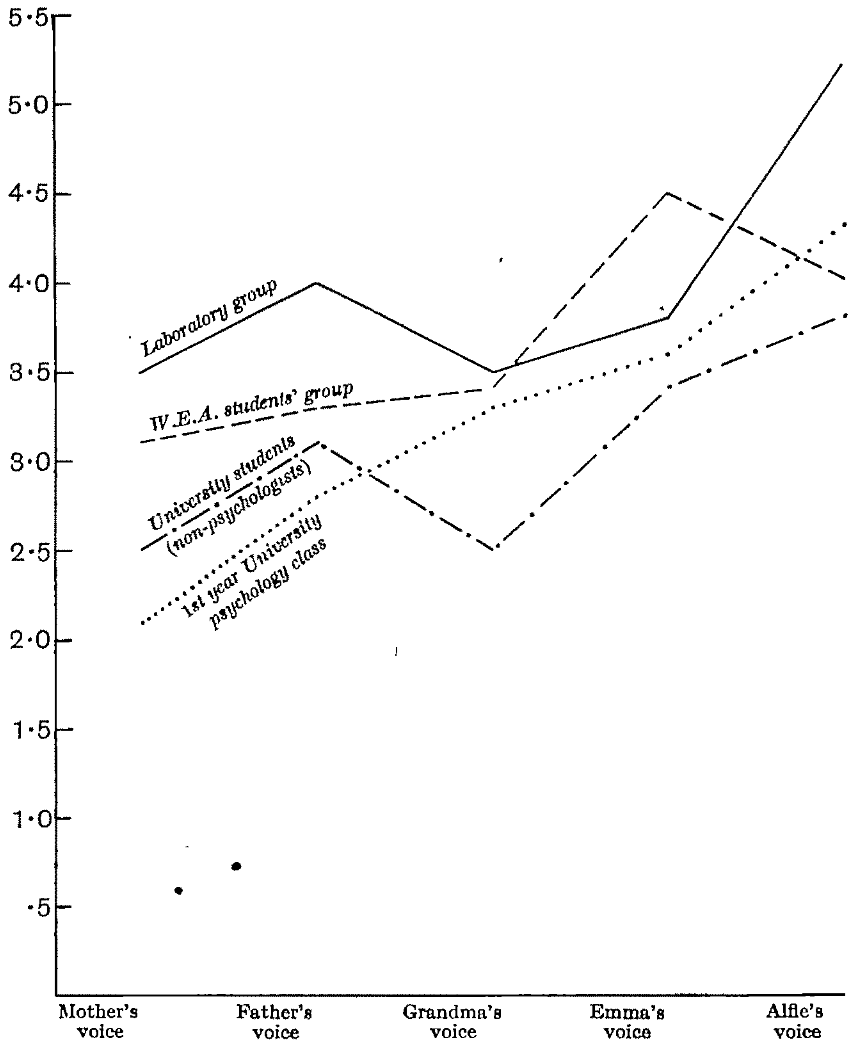
[1] *Social Psychology*, Boston, 1924.



Graph illustrating the different ratings of vividness of auditory imagery given by various groups of listeners. (Answers to question 1. Record *A*. This question was identical with question 1 on page 45.).

An attempt was made to correlate the number of years of musical training which the listeners had received, with their imagery rating, but the result was insignificant.

(*b*) *Synaesthesia*[1].

No less than 24 per cent. of the subjects reported some form of synaesthesia. From record *A*, there were 3 instances of colour synaesthesia and 15 of 'quality' or texture synaesthesia. Here are typical examples:

"The mother's voice was like the feel of the wires on some instrument like a mandolin."

"I should describe the voice of the mother as blue; the father seemed to be surrounded by a yellow haze."

In the answers to record *B* there were 10 colour synaesthesias and 17 quality or texture synaesthesias. The woman's voice was described as ranging from red-brown to yellowish white, and the man's from dark purple through grey to black. It is interesting to note that for each of these two voices the colours given are limited to definite parts of the spectrum.

In the experiments with both records a comparison was made between the vividness of imagery (visual and auditory) and the tendency towards synaesthesia. For record *A* the average vividness of auditory imagery for synaesthetic subjects was 2·57; the group average being 2·65.

From record *A* only 1 synaesthetic subject had a really vivid visual image, and 5 only moderately vivid. This result gives no evidence that synaesthesia is confined to people with vivid visual or auditory imagery, or is correlated with vividness of imagery.

The following seems to be modified synaesthesia[2]: "The voices seem to have colour only in so far as they determine the colour of the speakers. May was light brown, rather pale; Alfred rather coarse. Colours were not definitely those of hair, but obviously refer more to hair than to complexion."

(*c*) *Emotional reactions*.

In the answers to record *A*, 60·7 per cent. of the subjects replied that they were moved emotionally, or at least that the record was realistic, and 34 per cent. thought it artificial. In the answers to record *B*, where the real unpleasantness of the plot made emotional reactions more probable, 88·3 per cent. were moved emotionally, only 11·6 per cent. thinking it artificial. These answers are evidence that the investigation had succeeded:

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[1] For recent writings upon this subject see A. Argelander, *Das Farbenhören und der synästhetische Faktor der Wahrnehmung*, Jena, 1927 (bibliography of 446 titles); G. Anschütz, *Das Farbe-Ton-Problem im psychischen Gesamtbereich*, Halle, 1929. P. E. Vernon. "Synaesthesia in Music," *Psyche*, x, 1930; A. Weller, "Zur Geschichte und Kritik der Synaesthesie-Forschung," *Arch. f. d. ges. Psych.* 1931, LXXIX, 325–84 (with bibliography).

[2] Cf. Anschütz, *op. cit.* pp. 8–16.

(1) As drama, rousing the subjects from a state of indifference even in experimental conditions.

(2) As an 'unschematic' way of evoking imagery.

(*d*) *Individual partialities*[1].

The way in which prejudice, or so-called chance association of ideas, may influence the listener's choice can be seen in the answers to the question about the Cockney (Walworth) accent. From record *A* it was found that 31·2 per cent. liked this accent, and 64·6 per cent. disliked it. There were many teachers among the listeners, and this may account for the intensity of the prejudices.

The Cockney accent was disliked because it is: somewhat insincere (possibly because heard at theatres); associated with people disliked; slightly nasal; going too far round to get there; drawling, pitched too high in the female; too conceited; a deviation from normal intonation; appearing to affect superiority; vulgar; jars on ears; associated with living in London (disliked); popularly associated with costermongers, etc.; too much distorted, connected with rather bad comedy; not standard English; not full and rich; like nothing on earth;

"False and fair and smiling is the South,
Dark and true and tender is the North."

The Cockney accent was liked because it was: ugly but friendly; familiar; pleasant for a short time when not used too strongly; associated with people living in London; clear; a refreshing change; associated with resourceful type of person; connected with a music-hall comedian; every vowel contains about five different shades of emotion; a more enjoyable caricature of good English than the Lancashire dialect with its painstaking pronunciation of words like *congratulate*; associated with books containing Cockney humour; bright and business-like.

From record *A*, 52·3 per cent. of those who liked the Cockney accent retained it in their imagery, and 53·8 per cent. of those disliking it also retained it. From record *B*, 55 per cent. of those liking it, and 41·6 per cent. of those disliking it, retained it in their imagery. This question was asked too soon after hearing the record to find out if sounds which are liked are remembered for a longer time than sounds which are disliked, an interesting question of importance for psycho-analysts.

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[1] Cf. *Voice and Personality*, chapters VI and VII.

(e) *'Irrelevant' and 'discrepant' imagery.*

In relation to modern discussion of the relevance of imagery, especially visual[1], to certain mental functions, answers to the question whether the subjects had any irrelevant or discrepant imagery may be interesting. The following images are reported of persons or things not mentioned in the duologue of record *B*:

> A back room, leading into a shop, much better lighted.
>
> A room in a station hotel.
>
> Imagined myself buried alive.
>
> Vivid image of a red carpet.
>
> I had images of the (above) schoolmistress because of the similarity of her voice and the woman's voice.
>
> I could see Alfred's wife, his suburban villa, the scene at his home. Again, Alfred did not appear to stay in the office after the safe closed. I saw him walking home along a sunlit tramroad, which was not crowded.
>
> When the woman mentioned suffocation, image of supply of oxygen, as a volume.

Here, as elsewhere, it would be difficult to say if *any* imagery coming up in such a context was irrelevant.

(f) *Detailed and vivid imagery.*

Descriptions of vivid visual imagery were given. Of the experiences from record *B*, there was a general similarity between the detailed accounts.

> The woman was usually described as about 30, with untidy hair, blouse and skirt, and some remnants of good looks. The man was rather older, stout, florid, shifty-eyed and furtive.

The following quotations describe different types of imagery produced by the voices in record *A*:

> Mother small, harassed, very untidy hair, yet has time to coax the boy. Father is out of work, shirt sleeves, blue collar if any. The grandmother small, grey hair, watery blue eyes. The girl probably an unpleasant child with a habit of sniffing and fidgetting, pigtails. The boy probably the male counterpart of the girl.
>
> The mother (visualized) as a big mouth and two wrinkles, one on each side; the father as a moustache and overalls; the boy as thin white legs, white socks and black slippers.

With record *A*, 49 subjects, when they visualize, see the faces of the actors; 20 do not. With record *B*, 92 subjects see the actors' faces; 26 do not. In record *A*, 49 see the whole of the actors' bodies; 9 do not.

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[1] Cf. a symposium "The relevance of visual imagery to the process of thinking," by F. Aveling, F. C. Bartlett and T. H. Pear. This *Journal*, 1927, XXVIII, 1 f.

In record *B*, 55 see the whole of their bodies; 8 do not. In times of dramatic tension the faces were visualized more clearly. If some action was being performed, the appropriate limbs were focused more clearly.

With record *A*, 28 per cent. of the subjects thought that their images of the faces were new, 25 per cent. that they were based upon actual persons, and 46 per cent. that they were based on composites of several people. In record *B*, 32·1 per cent. thought that their images were new, 24·1 per cent. that they were based on actual persons, and 28 per cent. that they were composite.

The majority of listeners to record *A* visualized more of the events at the time of writing than when the record was being 'played.' In record *B* most of the events were visualized at the time by the majority of subjects. In view of a common belief amongst philosophers that minds trained to think lose to a considerable degree the power to visualize, not only when thinking abstractly but even when remembering, it might be emphasized that of 112 university graduates with very different training and interests, nearly all visualized the actors and events in this drama.

(*g*) *Vividness of auditory imagery and criticism of one's own voice.*

The majority of listeners to record *B* were critical of some aspect of their own voices. Several said that they had consciously changed their accents. The average intensity of auditory imagery for those critical of their own voices was 2·78; the average for those not critical, 2·76. Only 8·03 per cent. had not tried to alter their voices at some time, and 20 out of 112 said that they easily acquired accents, inflections and phrases. The average intensity of auditory imagery for those acquiring accents easily was 2·72, for those not acquiring accent easily 2·79. The two numerical differences in this paragraph are obviously not significant.

Of the listeners to record *B*, 54 were critical of imitations of dialect by friends or actors, while 12 were not. From examination of the detailed replies, the general impression can be gained that such subjects criticized all dialects of which they had any knowledge.

(*h*) *Attitude of listeners towards radio drama.*

From the record *A* results, it was found that 36 liked radio drama in general and 22 disliked it. The average vividness of auditory imagery for those liking it was 3·63; for those disliking it 2·17. The significance of the difference was 8·7. This would suggest that listeners who dislike radio drama have more vivid auditory imagery than those who like it.

There were 7 subjects with very vivid visual imagery who liked radio drama and 3 who disliked it. Out of 36 subjects liking radio drama 32 have visual imagery with varying degrees of vividness, while 17 out of the 22 who dislike radio drama also have visual imagery.

Of the listeners to record *B*, 48 liked radio drama and 20 did not. The average intensity of auditory imagery for those liking is 2·33, for those not liking 2·61, a relation the reverse of that in the results of record *A*. Out of 32 very vivid describers of visual imagery 18 enjoy radio drama and 7 do not. The percentage of visualizers enjoying radio drama is 61·2. That visual imagery seems favourable to the enjoyment of radio drama is therefore indicated by these figures.

The main reasons given for enjoyment were that the actors could be visualized as the subjects wished them to be; the listeners could provide their own settings, and outside influences, such as those that disturb one in the theatre, could be excluded. Radio drama was disliked because of the discordant element in the voices; because too much concentration of attention was required; because it was easy to lose the thread of a plot; because 'atmosphere' was lacking; and because the voices in the play did not differ enough from each other. No significant differences of opinion were traceable to differences of sex or of occupation.

The chief suggestions given for improving radio drama were that 'noises off' should be more realistic, characters should be exaggerated, there should be more clarity and variety of voice, slower speaking and more silences. The plays should be of action or of swift dialogue; there should be no dialect plays[1]. 'Effects' should be abolished, 'effects' should be intensified[2], and radio drama should be heard in the dark.

VI. CONCLUSIONS.

The following main conclusions seem justified by this investigation:

1. People with little or no psychological training tend to over-rate the intensity of their imagery.

2. The results from both records indicate that people who like radio drama have more vivid visual imagery than those who dislike it. No unambiguous conclusions concerning the vividness of auditory imagery were justified.

3. Synaesthesia, provoked by these 'dramatic' situations, more unusual, interesting, and emotional than have been presented before in laboratories, is much more frequent than when produced by the previous

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[1] Cf. *Voice and Personality*, pp. 58–71.

[2] Cf. "The Use of Sound Effects," *B.B.C. Year Book, 1931*, pp. 194–7.

methods. It seems to play an important part in the perception and memory, as well as the thinking, of its possessors.

4. As a method of obtaining introspections concerning imagery and synaesthesia, this test succeeded. The subjects were not offered a completely new situation, since most of them were familiar with 'listening in' and with the gramophone. This considerably increased the naturalness of their attitude.

5. There should be no confusion between the aims of this experiment and those of a 'thought experiment' in the sense of the Würzburg School. The listener is perceiving or remembering, not thinking. It is even arguable that he is not "perceiving with incomplete data," since a voice on the telephone offers fewer, and more distorted, sensory data—and we may know many voices on the wireless or the telephone without ever seeing their possessors or hearing their 'real' voices.

6. This experiment emphasizes the importance of prejudice in judging voices and dialects, and throws interesting light upon the attitude of one part of this country to another. This question of social psychology is not difficult to investigate.

7. This experiment shows that university graduates, who presumably are capable of abstract thinking, need not necessarily lose the power of vivid visualization.

8. Without exhaustive analysis—and often, too, psycho-analysis—much apparently irrelevant imagery cannot seriously be so named. Since most listeners described their imagery as composite, seemingly irrelevant imagery might be caused by the temporary entrance into consciousness of ideas associated with different components of the composite image. These ideas may be unclear, unidentified, but accompanied by an intense emotional tone[1].

The experiments described above give results which make it more futile than ever to pronounce off-hand that a particular image is irrelevant or discrepant. A welcome gift to experimental psychology would be a definition of 'relevant imagery' by a philosopher, if one can be found, who still retains vestiges of concrete visual and auditory imagery in his waking—and thinking—life.

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[1] Cf. *Voice and Personality*, pp. 102–3.

(*Manuscript received* 15 *December*, 1930.)

A NOTE ON THE REPETITION OF A SIMPLE MOTOR TEST.

By J. N. LANGDON.

I. *Reliability and diagnostic value* (pp. 55–57).
II. *Tables of measures derived from the repetition of two tests* (pp. 57–58).
III. *Summary of main features appearing in the tables* (pp. 58–61).

I. Reliability and diagnostic value.

The repetition of any test will normally produce a certain amount of improvement, due partly to the fact that the subjects will be better adjusted to the test conditions, and partly because the ability under examination may have increased as the result of its previous exercise. It is improvement of the latter type that creates a special problem in motor tests, in which the effects of practice are frequently both pronounced and prolonged.

The results of any such test may be made unreliable by either or both of two main causes. In the first case, the test may be so susceptible to chance and uncontrolled conditions that there is little correlation between any two performances. Secondly, the subjects may be so diversely affected by the practice given in each repetition, that they fail to preserve approximately the same relative rank in successive trials.

In current practice it is customary to estimate the effect of both these factors by repeating a test several times, and calculating the correlation between each trial. In this way the reliability of the test is said to be established. It seems, however, that this measure of reliability may possibly be misleading when it is derived from the successive repetition of a test on the same occasion, as sometimes appears to be the case. Such a procedure measures reliability under the most favourable conditions, as, on any single occasion, the variables of a personal nature are likely to be fairly constant, as are also those variables that are due to environment in the widest sense. So, when reliability is estimated in this way, there is an implicit assumption that these wider and more general factors are negligible in comparison with the intrinsic sources of variability. While it is quite possible that this is true, it seems that the assumption is sometimes made without any basis of experimental evidence. A measure of reliability that would be free from this objection could be obtained by the repetition of a test over a long period at widely separated intervals. From such results, keeping the immediate conditions constant, we should gain a better idea of the degree to which a test is influenced by those variables that usually lie outside experimental control, at least in educational or industrial testing.

In addition to the measure of reliability, there is the determination of the diagnostic value of any test. A satisfactory coefficient of reliability merely ensures that the test measures the ability within the limits of the practice period between the first and last 'reliability' trials. The test will only have any further diagnostic value if there is high correlation between initial and more fully practised ability in the performance tested, and this is a matter that requires separate determination in each case. Of course, the diagnostic value of a test may more confidently be assumed if the 'reliability' trials are numerous as well as consistent, but such an assumption must be very speculative if made from two or three trials only.

It will be noted that the use here of the term 'diagnostic value' does not involve any reference to a possible relationship between the test and any more complex ability. It refers only to the validity with which more practised performances may be inferred from those at an earlier stage of practice, and is thus analogous to the 'sample' method in industrial testing. At the same time, it seems unlikely that early performance in a test that fails to indicate its own more practised ability, can possibly be any more successful in indicating potential ability in any other operation.

It seems, therefore, that for each test it might be profitable to determine the reliability by numerous repetitions on widely separated occasions, and that the diagnostic value should also be determined by repetition until the final stage of practice is approached. A test, or group of tests, made at this fully practised period would then provide the means of measuring the relative diagnostic value (and incidentally, the reliability) of each test during the earlier stages of practice.

There are usually practical difficulties in the way of such a procedure[1], and consequently, reports of experiments carried right to the final stages of practice are exceedingly rare. Where it is possible to give trials on a limited number of occasions only, so that no measure of completely practised ability is available, some substitute criterion would be useful. This measure should be chosen so that it is influenced as little as possible by the conditions prevailing on any particular occasion. It is suggested that the total score of each subject in all trials may be a suitable measure.

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[1] An account of an experiment in which seven tests (nearly all more complex than those considered here) were repeated until practice was really final, is given by H. L. Hollingworth, in his book *Vocational Psychology*, New York, 1920. A review and criticism of Hollingworth's conclusions is to be found in an article by B. Muscio, "Motor capacity with special reference to vocational guidance," *Brit. J. Psychol.* XIII, 1922.

Two other measures of considerable interest are also available. One of these, the highest score made by each subject at any single trial, may be taken to indicate the relative ability under optimal conditions. It may be objected that this score depends on the degree of practice. This is true within limits, but analysis of the actual figures shows that the best performance of each individual is not necessarily made at the final trial, although it is so made by the group as a whole. In one of the tests described here, the score in the final trial was exceeded or equalled in earlier trials by nine of the nineteen subjects concerned. In the other test, fifteen subjects scored more highly in one of the earlier trials than they did in the final performance. It is true that in nearly all cases the highest score occurred somewhere within the three or four most practised trials. It seems, therefore, that we are justified in taking the highest single score of each individual to represent her performance under the most favourable conditions to herself within a given stage of practice. In the same way, it is proposed to adopt the lowest score made by each subject within the same narrow range of practice, as a measure of her ability under the least favourable conditions within the same period. Both measures are employed in the accompanying tables, and their relation to each other and to average performance is used as a rough indication of the influence of the more general sources of variation to which attention has already been directed.

II. TABLES OF MEASURES DERIVED FROM THE REPETITION OF TWO TESTS.

The tables show measures obtained from the scores made by nineteen subjects in each of a series of trials at a simple motor test. The trials occurred at irregular intervals on the dates shown. The groups were not absolutely identical, seventeen subjects being common to each. The subjects were schoolgirls aged about fourteen years. Their attitude to the testing was distinctly favourable, giving every appearance of maximum endeavour.

Table I shows the results of tests in 'knotting.' In this test the subjects were given a number of small pieces of string, each about

3 inches in length and of comparable tension. They were told to tie a single knot in as many pieces as possible. The time allowed was 3 minutes.

Table II shows the results of tests in 'match insertion.' Each subject was given a box of Swan Vestas with the heads removed, and was told to take the matches one at a time from the box and insert them into the small holes in the board provided. The time allowed was 90 seconds.

III. SUMMARY OF MAIN FEATURES APPEARING IN THE TABLES.

(*a*) In each test there is marked improvement, although the practice periods were very short and widely separated. This is particularly noticeable in the second trial in knotting. Here, a single trial of 3 minutes, given thirty-five days previously, has apparently effected a mean improvement of 8·4 per cent. It seems probable, therefore, that conclusions based on this type of testing may be very ill-founded if there is any inequality in the subjects' previous experience of such tests, even though the previous testing may have been short and remote.

(*b*) The coefficient of variation has been calculated for each trial. It will be noticed that the variation is greater in the first than on any subsequent occasion. A comparison of the mean variability for three successive stages of practice, shows a small but regular decrease with practice. In each test, the variability is lowest in the series composed of the highest single score of each subject. If this is significant and may be interpreted at its face value, it appears that variability under optimal conditions is likely to be less than usual. It must be remembered, however, that this calculation of variability takes no account of the probably greater value of each unit of achievement in the more practised trials.

(*c*) The correlation coefficients show that there is a tendency for each trial to be more intimately related to those trials near itself in respect of time, than to those more distantly removed. This difference, however, is not very great, and there are many exceptions that indicate the effect of chance factors.

(*d*) The correlation between the highest single score and the total score is high in each case. This may be taken to indicate a close connection between performance under normal and under optimal conditions[1]. This correlation is only slightly superior to that shown between

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[1] This is perhaps analogous to a similar relationship discovered by S. Wyatt to hold under very different conditions, between average and maximum rates of working of industrial operatives. "Maximum capacity and average achievement," *Brit. J. Psychol.* xx, 1930.

Table I.

Date	9/12/29	13/1/30	27/1/30	10/2/30	3/3/30	24/3/30	31/3/30	7/4/30	14/4/30	28/4/30	5/5/30	12/5/30	19/5/30	16/6/30	23/6/30	Highest single score	Total score	Lowest score in trials 12, 13, 14 or 15
Trial	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15			
Mean score of group	100·0	108·4	113·8	111·4	122·4	123·7	124·0	127·9	129·2	130·1	134·8	135·5	135·3	140·1	144·0	146·7	—	132·4
Coefficient of variation	15·3	12·7	9·9	11·0	13·0	13·3	11·0	10·3	12·3	11·7	11 1	11·5	12·5	9·8	11·1	9·6	—	12·0
			mean = 12 4					mean = 11·7					mean = 11·2					
Mean correlation with 1, 2, 3, 4 and 5	0·76	0·78	0·83	0·73	0·81	0·82	0·80	0·72	0·84	0·75	0·73	0·68	0·66	0·73	0·62			
			mean = 0·78					mean = 0·79					mean = 0·68					
Mean correlation with 6, 7, 8, 9 and 10	0·76	0.82	0·83	0·72	0·81	0·84	0·82	0·73	0·81	0·79	0·73	0·76	0·75	0·74	0·72			
			mean = 0·79					mean = 0·80					mean = 0·74					
Mean correlation with 11, 12, 13, 14 and 15	0·70	0 71	0·73	0·68	0·60	0·75	0·86	0·73	0·76	0·62	0·83	0·83	0·82	0·79	0·79			
			mean = 0·68					mean = 0·74					mean = 0·81					
Correlation with highest single score	0·62	0 71	0·82	0·72	0·61	0·76	0·86	0·81	0·79	0·66	0·84	0 81	0·88	0 87	0·95	—	0·86	0 81
Correlation with total score	0·85	0·93	0·93	0·82	0·86	0·93	0·94	0·83	0·93	0·83	0·84	0·82	0·82	0·85	0·79	0·86	—	0·85

Correlation between mean of first two trials and mean of last two trials = +0 70.
Correlation between mean of first two trials and absolute gain in mean of last two trials = −0·27.
Correlation between mean of first two trials and percentage gain in mean of last two trials = −0·72.

Table II.

Date	27/1/30	10/2/30	24/2/30	3/3/30	24/3/30	31/3/30	7/4/30	14/4/30	28/4/30	5/5/30	12/5/30	19/5/30	16/6/30	23/6/30	Highest single score	Total score	Lowest score in trials 12, 13 or 14
Trial	1	2	3	4	5	6	7	8	9	10	11	12	13	14			
Mean score of group	100·0	109·1	112·4	122·8	119·9	126·2	128·4	127·7	127·7	132·5	131·6	132·8	134·9	136·4	142·1	—	128·4
Coefficient of variation	10·2	8·5	7·4	10·0	9·6	8·5	7·8	9·1	7·8	8·7	7·8	7·2	6·7	7·7	5·2	—	7·9
			mean = 9·1					mean = 8·4				mean = 7·4					
Mean correlation with 1, 2, 3, 4 and 5	0·43	0·65	0·62	0·72	0·51	0·50	0·58	0·71	0·51	0·64	0·63	0·44	0·44	0·48			
			mean = 0·59					mean = 0·59				mean = 0·50					
Mean correlation with 6, 7, 8, 9 and 10	0·54	0·58	0·59	0·53	0·60	0·49	0·61	0·59	0·47	0·52	0·63	0·44	0·54	0·52			
			mean = 0·57					mean = 0·54				mean = 0·53					
Mean correlation with 11, 12, 13 and 14	0·44	0·44	0·56	0·55	0·48	0·59	0·67	0·60	0·42	0·39	0·57	0·57	0·49	0·61			
			mean = 0·49					mean = 0·53				mean = 0·55					
Correlation with highest single score	0·51	0·57	0·77	0·72	0·77	0·68	0·73	0·70	0·49	0·69	0·70	0·55	0·67	0·77	—	0·88	0·67
Correlation with total score	0·64	0·71	0·81	0·82	0·77	0·69	0·85	0·89	0·66	0·75	0·84	0·66	0·68	0·72	0·88	—	0·74

Correlation between mean of first two trials and mean of last two trials = + 0·40.
Correlation between mean of first two trials and absolute gain in mean of last two trials = − 0·63.
Correlation between mean of first two trials and percentage gain in mean of last two trials = − 0·77.

the total score and the lowest score made subsequently to the eleventh trial. Within this stage of practice, the series composed of the lowest single scores made by each subject is also found to be quite highly related to the series composed of highest single scores. From these measures of relationship it seems reasonable to conclude that, so far as these tests are concerned, those wider factors that influence performance from one occasion to another, are not sufficiently strong to introduce any serious disturbance into the relative rankings of the subjects.

(*e*) The relation between the first and last performances in knotting is quite high, and is nearly double that in match-insertion.

(*f*) In each case there is negative correlation between initial performance and absolute gain in the two final performances. The negative relationship is still more marked between initial performance and percentage improvement. This result might be altered if means could be found to give appropriate weight to the more practised units of achievement. So far as the actual scores are concerned, it is in agreement with the decrease in variability in the more practised stages.

(*g*) If the total score is adopted as a suitable measure of ability within the period of observation, then the correlation between this and each trial indicates the value of each successive trial as a measuring instrument. In the case of knotting there is no great difference in this respect. Even the first trial appears to be a reasonably good measure. In match insertion, the first trial is slightly inferior to the later performances, while all are less satisfactory than in knotting. This is partly because the match insertion test contains greater intrinsic sources of variation. Higher correlations would almost certainly have been obtained if it had been found possible to repeat the test two or three times on each occasion, and adopt the mean or median score. However, despite this weakness, the figures in the two tables seem to show a satisfactory consistency so far as general tendencies are concerned.

The writer desires to express his thanks to Miss M. Clews, Head Mistress of the School at which the tests were made; to the Director of Education for Coventry, through whose kindness the experiment was made possible; and to Mr S. Wyatt, whose constructive criticism has been much appreciated.

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THE EFFECT OF CHANGE OF REWARD ON LEARNING IN CHICKENS.

By A. P. CHATTOCK and G. C. GRINDLEY.

(*University of Bristol.*)

I. *Introductory* (p. 62).
II. *Apparatus and method* (pp. 62–63).
III. *Quantitative results* (pp. 63–65).
IV. *Behaviour of the chicks at the change of reward* (p. 65).
V. *Conclusion* (p. 66).

I. INTRODUCTORY.

In a paper published in 1928 Elliot (1) has given evidence that in maze learning in rats a change in the kind of food given as a reward is followed by a partial loss of the maze habit. Thus Elliot found that if rats are learning a maze for a reward of bran mash, and during the learning the reward is changed to one of sunflower seed, then the learning curve, which has been falling gradually before the change, rises rapidly after the change and then falls again.

The experiments described in this paper were designed to test whether a similar effect could be found in young chickens. A very simple kind of maze was used, and boiled rice and a mixture of chick seed and groaten were given as the two kinds of reward.

After the work had been begun, a further paper by Elliot (2) appeared in which he gave evidence that the effect which he had previously found was due, not directly to the change in the kind of reward, but to the fact that at the time of the change the rats had not developed a sufficient appetite for the second kind of food.

II. APPARATUS AND METHOD.

A plan of the apparatus is given in Fig. 1. The run *ABCD* was about 2 ft. × 2½ ft., and was surrounded by walls 1 ft. high. The chicks started from the release box *R*, and found food at *F*, which could not be seen until they reached the point *P*. *GG* was a vertical glass screen, and behind it were placed two "decoy" chicks *S*, in order to prevent the chick which was running the maze from becoming 'lonely.' The decoy chicks were not taught the maze.

Two groups, each of ten chicks, were used in the first series of experiments: they will be referred to as Group A and Group B. Each chick ran the maze once on each day, beginning on the fifth day after hatching, at a time when it had been without food for about fifteen hours. For the first five days the chicks of Group B received a reward of six grains of boiled rice, and those of Group A a reward of a similar quantity of a mixture of chick seed and groaten. On the sixth day the rewards were interchanged, so that Group A now received the rice and Group B the mixture. The experiment was continued for five more days.

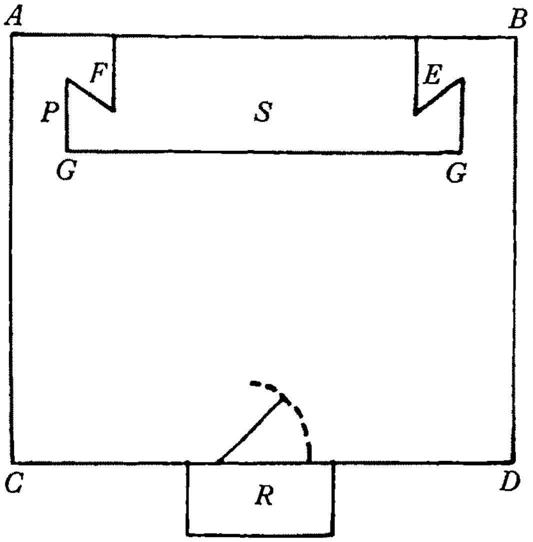

Fig. 1.

At each trial the chicks were timed from the moment at which they left the release box to the moment at which they reached the point *P*, and each chick was given a score equal to log 100/*t*, where *t* was this time in seconds.

III. QUANTITATIVE RESULTS.

Four of the chicks of Group A and five of those of Group B showed little if any evidence of learning, owing, apparently, to a distaste for the kind of food given as a reward; and the results obtained with these nine chicks were therefore neglected.

The results with the others (six of Group A and five of Group B) are given in Fig. 2, in which the average score of the two groups are plotted separately above, and their mean *C* is plotted below to a different zero. The circles mark the day on which the reward was changed.

The experiment was then repeated with another hatch of chicks. In this case there were seven chicks in Group A and nine in Group B. The experiment began on the ninth day after hatching and continued for eight days before and eight days after the change of reward.

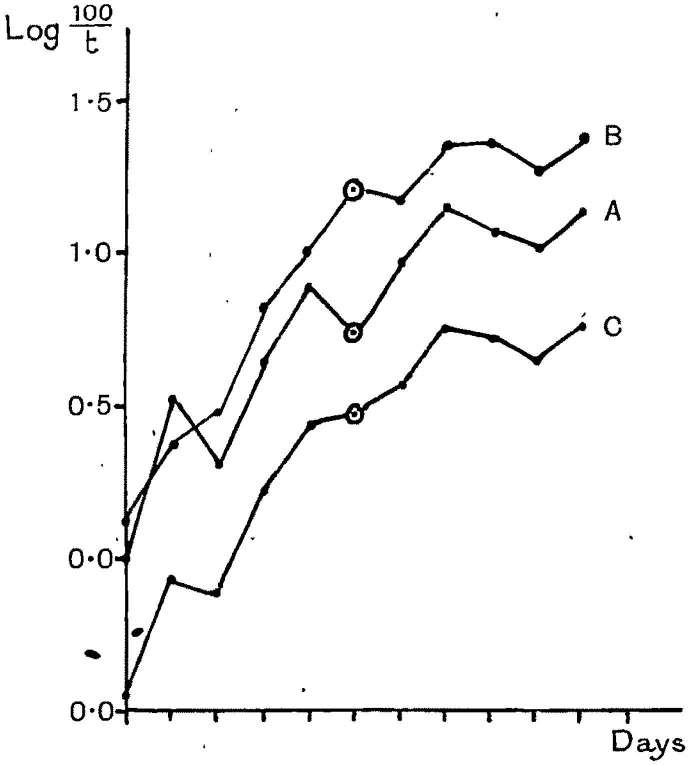

Fig. 2.

The average scores (*A*, *B*) of Groups A and B are shown in the upper part of Fig. 3, and their mean, *C*, below. The sixteen chicks concerned all ate the reward satisfactorily both before and after the change.

In addition to these sixteen chicks, three '*A*' chicks and one '*B*' chick ate little or none of the new reward for two or three days after the change, but afterwards ate satisfactorily. The mean score of these four chicks is shown by the dotted line *D* in Fig. 3, which is plotted to the same zero as *C*, so that it may be compared with the mean curve for chicks which ate normally both before and after the change.

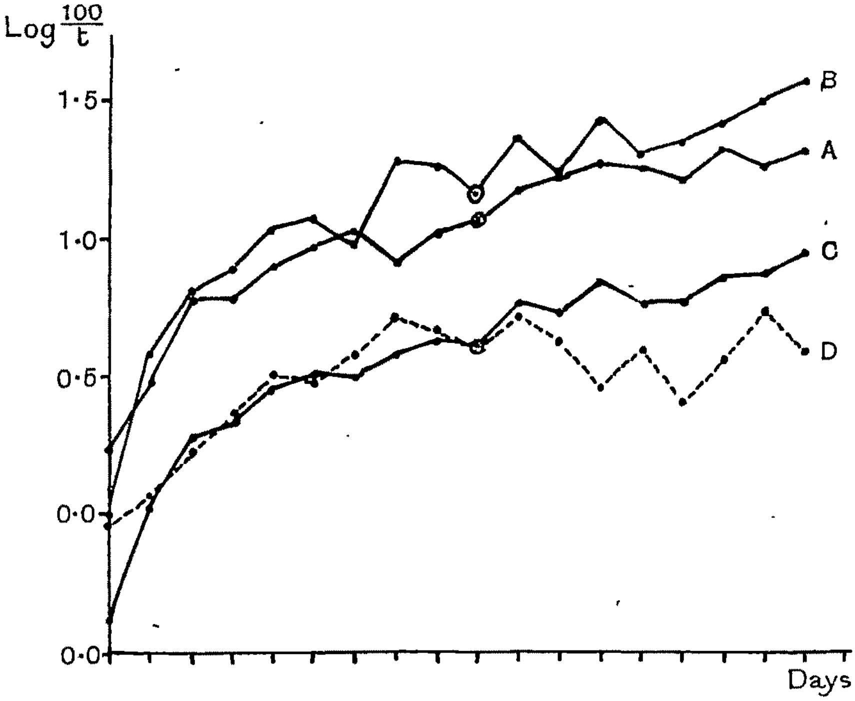

Fig. 3.

IV. BEHAVIOUR OF THE CHICKS AT THE CHANGE OF REWARD.

On the day when the reward was changed many of the chicks appeared to be very much surprised at the sight of the new kind of food. This was shown in most cases by marked hesitation before pecking, often accompanied by excited chirping; while a few of the chicks at the sight of the new reward ran in and out of the food box several times shrieking loudly before they pecked; and one or two searched the dummy food box (*E* in Fig. 1)—where no food was ever placed—as though to see whether by chance the proper kind of food was there. But all the chicks which formed Groups A and B finally ate the whole of the new kind of food, and on the second or third day after the change they showed little if any sign of surprise.

V. CONCLUSION.

The curves given here, with the exception of the dotted curve *D* (in Fig. 3), afford no evidence of the effect noted by Elliot in his earlier paper (1); that is to say a change of reward which was sufficient to produce marked signs of surprise produced no significant effect on the gradual rise of the learning curves. Curve *D*, however, which applies to chicks who at first disliked the new reward, shows some evidence of an effect of this kind.

The experiments described in this paper therefore support (for the case of chickens and the rewards used here) the conclusions arrived at by Elliot in his second paper (2).

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A SIMPLE MIRROR TACHISTOSCOPE.

By D. F. VINCENT and A. W. WOLTERS.

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The instrument figured in this article embodies an attempt to meet the many theoretical requirements of the perfect tachistoscope by constructing one without moving parts. It is a mirror tachistoscope, suitable for binocular vision, but the mirror is stationary, the alternation of the fixation and exposure fields being obtained by a switching device of a simple character.

Fig. 1 shows the optical arrangement of the tachistoscope. The exposure object (*C*) and the fixation field (*D*) are prepared upon sheets of translucent paper and illuminated from behind by the lamps *A* and *B*.

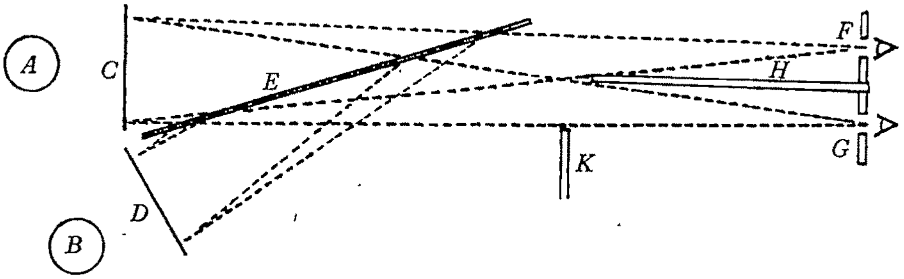

Fig. 1.

A sheet of plate-glass (*E*) is so placed that the image of *D* seen by reflection from the surface of the glass apparently coincides in plane and position with *C*. If the whole system is enclosed in a light-tight box, when one only of the lamps is alight only one field can be seen. To keep the perceived intensity of the fields constant during a series of experiments it is necessary to keep the distance of the eyes from the objects constant by means of the viewing mask *F*, *G*. The baffles *H* and *K* prevent a direct view of *D*. The correct position of *H* and *K* is important, for the brightness of *D* when viewed directly is much greater than when viewed by reflection. If a lateral direction of vision were permitted this superior brightness would cause distraction throughout, and a flicker at the moment of exposure. It is also imperative that the lamps *A* and *B* should be placed directly behind the objects or uneven illumination results. This gives rise to an interesting but irrelevant series of effects akin to the ϕ-phenomenon.

Fig. 2 shows the complete instrument, the lettering being the same as in Fig. 1. The sheet of plate-glass is fitted into a groove in a block of wood. To bring the image of *D* into lateral coincidence with *C* this block pivots at *L*, and is rotated through a few degrees by a pivoted arm (*MN*) which can be adjusted from outside the case by means of the milled head (*O*) and screw (*N*). This screw should be of coarse pitch for easy adjustment, a $\frac{3}{16}$ in. Whitworth thread being convenient. The exposure and fixation objects are carried in tin-plate slides which travel in tin-plate runners (*R*, *R*). The lamps are mounted on rectangular blocks of wood (*W*, *W*), and behind each lamp three strips of common glass mirror are placed (*T*, *T*, *T*). These reflectors we find sufficient to even up the illumination of the objects. The lamps are moved until a position is found giving the desired relation between the luminosities of the exposure and the pre- and post-exposure fields and then locked in position by the screws (*P*, *Q*). Lengths of $\frac{1}{8}$ in. × $\frac{1}{2}$ in. strip iron are used to distribute the pressure of the screws (*S*, *S*).

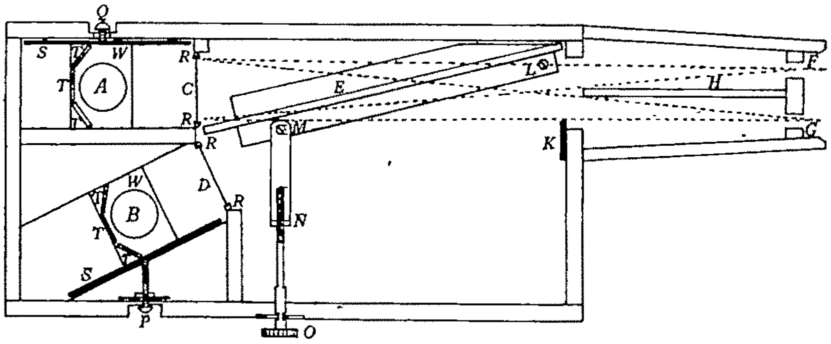

Fig. 2.

Fig. 3 shows one of the object slides. They are made from stout tin-plate. An aperture two inches square is cut in such a position that it will be directly in front of the lamp when the slide is in position, and strips of tin-plate, cut to an *L*-section, are soldered around the bottom and sides of the aperture to form an object holder. The knob at the top serves a double purpose; it is used to raise and lower the slide and also to adjust its position in the vertical direction. The knob carries a short length of studding and two nuts between which it is clamped to the projecting bracket. To bring the fields into vertical coincidence one of the slides must be adjustable in height. The depth to which the slide drops in its grooves depends upon the length of studding projecting beneath the bracket, as the end of the studding comes into contact with the top of the box.

Fig. 4 is an isometric view of the exterior of the tachistoscope. The viewing tunnel is made detachable and can be accommodated within the body of the instrument for transport. *X* and *Y* are lids hinging at *Z* to give access to the interior. The whole of the interior, except the lamp compartments, must be blackened to avoid trouble from reflected light. The height of the model described here is about eight inches, but this will depend upon the character of the lamps used and the types of lamp-holders fitted. The instrument works on the mains current, and vacuum lamps with a zig-zag filament are essential since the more compact filament of gas-filled lamps causes difficulty in securing even illumination. It should be observed that it is not possible to make an

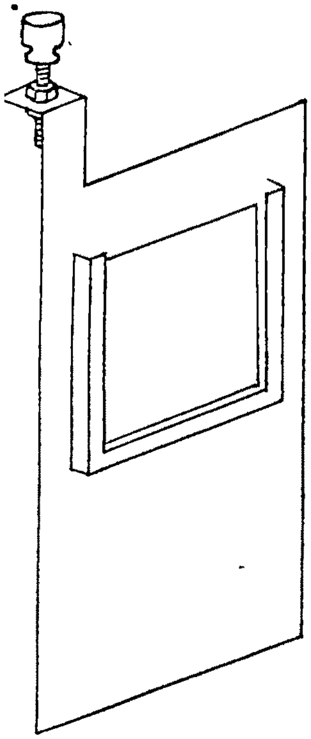

Fig. 3.

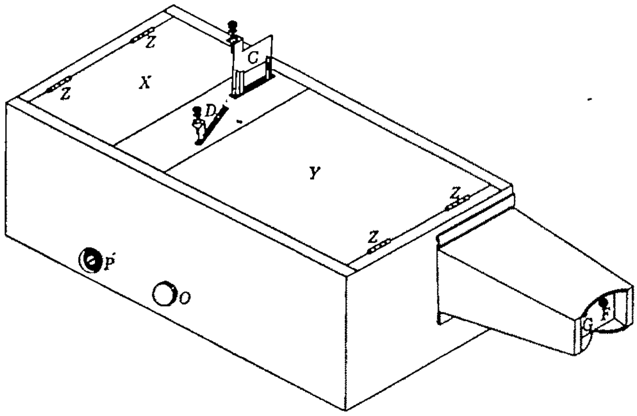

Fig. 4.

instrument much smaller than the one described if it is to be available for binocular vision, for the line of regard from each eye makes a different angle with the reflector, and as the dimensions of the tachistoscope are reduced the difference of angle increases. But a difference in angle makes a difference in the amount of light reflected. This difference becomes greater as the angle between the reflector and the line of regard becomes less acute. Consequently the position of the plate-glass is important, though not critical. An angle of about 15° to the long axis of the instrument is suitable.

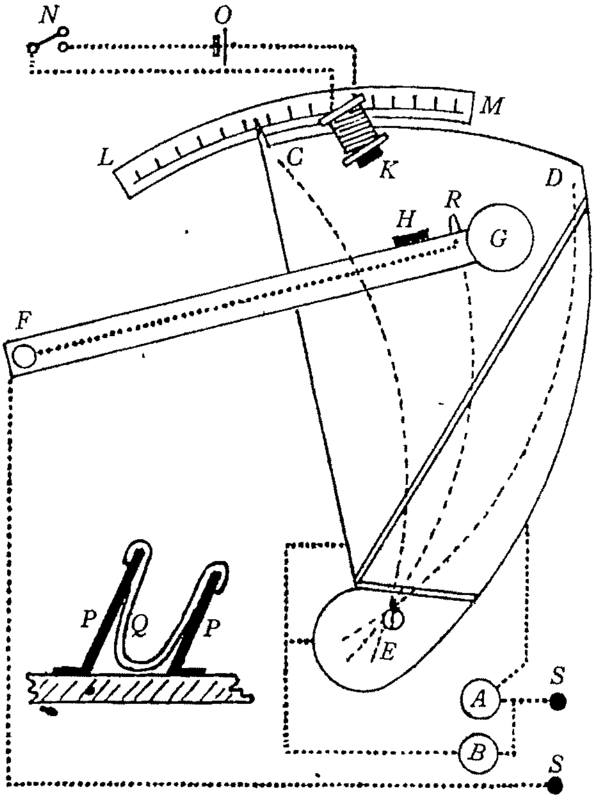

Fig. 5.

Fig. 5 shows the time switch for giving brief exposures. The electrical connections are shown by dotted lines, *SS* being the connections to the mains, and *AB* the lamps. *N*, a control switch, *O* a cell, and *K*, an electromagnet, together form an independent circuit for operating the time switch by distant control. *FG* is an arm pivoted at *F* and carrying a substantial weight at *C*, a spring contact at *R* and an iron armature at *H*. *CDE* is a sheet of hard wood pivoted at *E*, and bearing three sheets of metal fastened to its face: these are separated from each other by a gap of about a millimetre. When the arm is raised to its fullest extent the spring *R* makes contact with the upper sheet of metal and thereby closes the circuit, lighting the lamp behind the fixation object. As it falls it makes a rubbing contact with the other metal sheets, lighting up the lamp behind the exposure object as it crosses the central sheet, and relighting the other when it rests against the lower sheet. When *CDE* is rotated outwards to the right a shorter contact is made with the middle sheet, and the exposure period is consequently reduced. The dimensions of the time switch determine the maximum exposure possible, this being about a quarter-second in our model. The switch is easily calibrated for the arbitrary scale *LM*, and is extremely constant. Since the metal sheets are connected directly to the mains, certain obvious precautions must be taken to protect the operator. The weight must be trapped at the end of its fall to prevent a rebound causing a second exposure. The trap (*PQP*), which is shown on a larger scale, consists of two stout metal plates from which hangs a loop of thick felt. The plates are so placed that the weight, which is cylindrical, fixes itself into the loop.

Before commencing a series of exposures the lamps are adjusted. Opposite halves of the two fields are masked by opaque objects, both lamps switched on, and moved to and fro until the desired equality or difference in illumination of the two halves is obtained. Then, with plain objects in the holders, coincidence of the fields is secured by means of the adjusting screws. The holders are made to take lantern slides, but objects are more conveniently prepared on any fairly opaque paper which is free of water-mark. We have found the paper commonly used for cyclostyle printing very suitable if the objects are prepared in ink or soft pencil. Care should be taken to keep the details in the centre of the field, and it is worth while to make templates of various sizes and write the objects through them. A faint pencil dot provides a sufficient fixation point, which leaves no after-image to complicate the exposure field.

The value of the Vincent tachistoscope depends mainly on two facts: first, since the tachistoscope proper is without moving parts, it is *absolutely* silent when operated by distant control, and secondly, the whole of the object is exposed simultaneously. To one accustomed to other types of instrument there is at first something disturbing in the silent appearance and disappearance of the stimulus, the rest of the field remaining unchanged. By using a hand switch the length of the exposures can be indefinitely increased, but a lower limit is set by the time taken to heat the filaments. This could probably be substantially reduced by arranging a circuit to preheat the filament of the exposure lamp to a point just below glow. We have not tried this, as the periodicity of our A.C. supply sets a lower limit. Within these limits it is a pleasant instrument to work with, the adjustments are simple and effective, and we have not detected any disturbing glow either at the beginning or at the end of the exposure, nor is there any trace of flicker when the precautions described above are taken.

If now the characteristics of the Vincent tachistoscope are compared with the classical list of requirements, it will be seen that this instrument, simple and inexpensive as it is, is of high scientific value. It is available for binocular vision, there is no disturbance of accommodation or adaptation, and, if care be taken in the preparation of the objects, the exposure may be presented as a modification of the fixation field.

A more portable model has been prepared, in which the tachistoscope, lamp batteries, and control apparatus all fit into a travelling case 8 in. × 6 in. × 4 in. This can only be employed in monocular vision, a lens being fitted in the eyepiece to secure accommodation. We do not profess that this is an instrument of high scientific precision, but it is convenient, and in some respects superior to any focal-plane shutter device.

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ANSWER-PATTERN AND SCORE-SCATTER IN TESTS AND EXAMINATIONS[1].

By DAVID A. WALKER.

(*From the Department of Education, University of Edinburgh.*)

I. *Introduction* (pp. 73–74).
II. *Answer-pattern and score-scatter* (p.74).
III. *'Hig' and 'Unig'* (pp. 74–76).
IV. *Occurrence of hig* (p. 76).
V. *Relation of hig and steepness* (p. 77).
VI. *Coefficient of hig* h (pp. 78–80).
VII. *The experiment* (pp. 80–82).
VIII. *Results* (pp. 82–85).
IX. *Conclusions* (pp. 85–86).

I. INTRODUCTION.

It is a well-known fact that it lies in the power of an examiner to obtain, within certain limits, whatever type of score-scatter he desires, and that he does this by a suitable choice of questions. The methods of choice are usually of the rule-of-thumb type, experience or intuition being the guide. In this paper an attempt is made to investigate the theoretical basis of the preordaining of the score-scatter, and an account is given of an experiment in which tests were designed on this theoretical basis to produce score-scatters of given types. As will be seen in the course of the paper, the problem is really a dual one: "(i) How, and (ii) how far, do the examiner's plans preordain the score-scatter?"

The examiner has control over the test and its details, such as the number of scoring points, difficulty of questions, time allowance, and so on. A factor over which he has no control, but which has a great effect on the type of score-scatter obtained, is the character of the group of testees. As being outside the examiner's control, this factor is largely ignored in the present investigation.

The type of test to be considered here consists of independent items, any one of which may be answered correctly independently of other questions. The difficulty of the individual question in relation to the difficulty of the other questions is then the main tool of the examiner in preordaining the score-scatter. In any individual test there may be other factors present, such as a linking of certain questions in a degree of interdependence, which will affect the score-scatter; but such factors which are peculiar to the single test in question are here ignored.

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[1] Condensed from a thesis submitted in part fulfilment of the requirements for the degree of Bachelor of Education.

II. ANSWER-PATTERN AND SCORE-SCATTER.

The varying degree of difficulty of the questions will be reflected in the different frequencies with which they are correctly answered; an easier question will be answered correctly more frequently than a harder question, and, if certain conditions later to be stated are fulfilled, this is the only test of degree of difficulty. If question 1 is answered correctly n_1 times, question 2 n_2 times, and so on, then these questions will be placed in order of difficulty when their n's are in order of magnitude. The table of values of n for the questions of a test, placed in order of magnitude, is called the 'answer-pattern' of that test. A test has not a unique answer-pattern, for the pattern depends not only on the difficulties of the items, but also on the character of the group of testees. The characteristic answer-pattern may be taken as that produced by a 'normal' selection, a fair sample, of the population.

A useful extension of this nomenclature is to let n_0 represent the number of candidates.

The capital letters N_x are used to denote the numbers of candidates scoring x marks exactly. It is obvious that the n's and the N's are related in some ways. It can readily be seen that

$$\sum_0^m N_x = n_0,$$

and that if m is the number of items in the test

$$\sum_1^m xN_x = \sum_1^m n_x.$$

In all subsequent work it is assumed that the questions have been placed in order of difficulty so that

$$n_0 > n_1 > n_2 > n_3 \ldots > n_m.$$

III. 'HIG' AND 'UNIG.'

A score of exactly x in a test may be made up in many different ways. The most probable composition is that obtained by answering the x easiest questions, but owing to individual differences not every score x is actually obtained in this unique manner. An element of 'higgledy-piggledyness' enters into the composition of all save zero, and perfect, scores, for which Prof. Godfrey Thomson has suggested the name 'hig,' and for its converse the term 'unig.' These terms are used throughout this paper. By a test being unig we mean that each score x is composed of correct answers to the x easiest questions, and therefore of correct answers to no other questions. Hig implies a departure from this composition. Note that it is not sufficient for our purposes to define unig by stipulating that every score x is identical in composition—there must be added the condition that it is composed of the x easiest items; in other words the score $x + 1$ always comprises the x items of the score x, and one more.

Now if hig is absent, that is each score is unig, it is easy to show that an *exact* relationship exists between the n's of the answer-pattern and the N's of the score-scatter. Symbolically

$$\left.\begin{array}{l} n_0 = N_0 + N_1 + N_2 + N_3 + \dots + N_m \\ n_1 = \phantom{N_0 + {}} N_1 + N_2 + N_3 + \dots + N_m \\ n_2 = \phantom{N_0 + N_1 + {}} N_2 + N_3 + \dots + N_m \\ \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \\ n_m = \phantom{N_0 + N_1 + N_2 + N_3 + \dots + {}} N_m \end{array}\right\} \quad \dots\dots(A),$$

or in another form

$$\left.\begin{array}{r} n_0 - n_1 = N_0 \\ n_1 - n_2 = N_1 \\ n_2 - n_3 = N_2 \\ \dots\dots\dots\dots\dots\dots\dots \\ n_{m-1} - n_m = N_{m-1} \\ n_m = N_m \end{array}\right\} \quad \dots\dots(B).$$

These equations show that when hig is absent a given answer-pattern completely determines the score-scatter. The relationship may be very neatly illustrated graphically; the difference of adjacent ordinates on the n or answer-pattern graph being equal to the corresponding ordinates of the N or score-scatter graph. In the limit, when there are innumerable items, equations (A) become

$$n_x = \int_x^\infty N dx,$$

equations (B) become $$-\frac{dn}{dx} = N_x.$$

From the above equations and their graphical representations a rather important conclusion may be drawn. The second set of equations may be interpreted as "the ordinates of the score-scatter curve are given by the *slope* of the answer-pattern." Thus to separate out candidates at the top by skewing the score-scatter positively, with the tail to the right, it is necessary to provide an answer-pattern falling sharply at first, and then falling slowly over the upper half of the items. That is, a relatively large number of difficult questions must be present, but *it is not necessary for any of the questions to be outstandingly difficult.* The spacing out of the candidates occurs as a result of the *differences* between adjacent questions being slight, not as a result of the absolute difficulty of any question. Two test papers of exactly the same range of difficulty may, by a proper choice of intermediate questions, skew the score-scatter in opposite directions.

IV. OCCURRENCE OF HIG.

The above exact statements refer to cases where hig is absent. But in actual practice, hig enters in varying degree into the composition of every test result. Due to individual variations in preference or ability for certain questions, all scores of x are not made up uniquely of answers to the x easiest items. Thus there is destroyed, in greater or less degree, the exactness of the relation established between answer-pattern and score-scatter, according as the amount of hig present is large or small.

The amount of hig present depends on various factors. One such, excluded here, is the linking of questions so that a correct answer to one is only possible after a correct answer to the preceding one. A second, having much the same effect, is the enlarging of the steps of difficulty between the questions. Then a correct answer to a difficult question will very probably be accompanied by a correct answer to the preceding question, since it is so much easier, and so on. On the other hand, a test whose questions are all of the same difficulty will tend to show a great degree of hig in the answering. These two statements will be proved mathematically below. Lastly, the injunction "Begin at the beginning and go straight through" usually found in group tests, will tend to decrease the amount of hig. This last factor is psychological and cannot easily be quantitatively investigated. The second factor mentioned deserves a more detailed investigation.

Since large steps of difficulty between questions will be shown in a steep answer-pattern graph, we may designate such a test as 'steep,' and the opposite type of test as 'flat.' This is not an exact definition, but it serves a useful purpose here. We shall now prove that 'steepness' decreases hig, while 'flatness' makes it a maximum.

V. RELATION OF HIG AND STEEPNESS.

Let the m questions of a test be arranged in order of descending difficulty and then numbered 1, 2, 3..., m. Let the *a priori* probability of answering correctly question a be p_a, and so on. Then the probability of scoring exactly x in unig fashion is

$$p_1 p_2 p_3 \ldots p_x (1 - p_{x+1}) (1 - p_{x+2}) \ldots (1 - p_m).$$

The probability that any given score x has been compiled in unig fashion is then

$$\frac{p_1 p_2 p_3 \ldots p_x (1 - p_{x+1}) (1 - p_{x+2}) \ldots (1 - p_m)}{\Sigma\, p_a p_b p_c \ldots p_e (1 - p_s) (1 - p_t) \ldots (1 - p_w)},$$

where each term of the denominator is composed of x p's and the corresponding $m - x$ $(1 - p)$'s multiplied together, and Σ denotes the sum of all the ${}_mC_x$ such terms, including that already appearing in the numerator. The numerator is the chance of getting x marks in unig fashion, the denominator is the sum of the chances of getting x marks in any fashion whatever, and their ratio is the chance that a given score x is actually unig.

Now p_a is evidently proportional to n_a; substituting and eliminating the constant of proportionality the probability, of any score being unig is seen to be

$$\frac{n_1 n_2 n_3 \ldots n_x (n_0 - n_{x+1}) (n_0 - n_{x+2}) \ldots (n_0 - n_m)}{\Sigma\, n_a n_b n_c \ldots n_e (n_0 - n_s) (n_0 - n_t) \ldots (n_0 - n_w)}, \text{ or } u_x.$$

The probability that a whole test is unig therefore is

$$\Pi = \prod_{x=1}^{x=m} u_x,$$

and the probability of hig in a test equals $1 - \Pi$.

Now in a flat test $n_1 = n_2 = n_3 = \ldots = n_m$. The probability of hig therefore becomes

$$1 - \Pi \frac{1}{{}_mC_x},$$

which rapidly approaches 1 or certainty as m increases. (For m equal to 3, $1 - \Pi$ equals 0·89.) That is, for a flat test the probability of hig is a maximum.

Similarly, in a steep test, it can be shown that the above product Π tends towards unity and therefore that the probability of hig tends to zero.

Therefore a steep test reduces the amount of hig, and a flat test gives the maximum amount of hig.

VI. COEFFICIENT OF HIG *h*.

Once the results of a test are known, the amount of incidence of hig can be measured. Hig represents a deviation from the exactness of the relations (B), and may therefore be measured by the amount of such deviation, or better by the sum of the squares of the deviations. Consider then

$$\sum_{0}^{m} (n_x - n_{x+1} - N_x)^2.$$

When hig is absent this equals zero, and conversely, when this is zero hig is absent. To convert this measure into a coefficient, divide by the maximum amount of hig. This, as shown above, occurs when

$$n_1 = n_2 = n_3 \ldots = n_m = n, \text{ say.}$$

Then

$$\sum_{0}^{m} (n_x - n_{x+1} - N_x)^2$$

becomes

$$\sum_{0}^{m} N_x^2 + n^2 + (n_0 - n)^2 - 2nN_m - 2(n_0 - n) N_0.$$

Using this as denominator, let h the coefficient of hig be defined[1] as

$$h = \frac{\sum_{0}^{m} (n_x - n_{x+1} - N_x)^2}{\sum_{0}^{m} N_x^2 + n^2 + (n_0 - n)^2 - 2nN_m - 2(n_0 - n) N_0}.$$

In the majority of tests discussed $N_0 = N_m = 0$, and

$$h = \frac{\sum_{0}^{m} (n_x - n_{x+1} - N_x)^2}{\sum_{1}^{m} N_x^2 + n^2 + (n_0 - n)^2}.$$

It may clarify the above symbolism and reasoning if one fictitious example be shown, in which the *a priori* probability of hig and the actual amount of hig present are calculated. The table shows the score sheet of the twelve candidates who sit a test comprising three questions (for $m > 3$ the calculation of *a priori* hig becomes very heavy). A cross in the square represents a correct answer by that candidate to the corresponding question.

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[1] Since the completion of this work and during the task of condensing the thesis into the present form, it has occurred to me that Pearson's measure of goodness of fit of the differential of the answer-pattern to the actual score-scatter would be a measure of 'hig,' and would be closely connected with the coefficient here employed; and I hope to explore this avenue.

	Question			
	1	2	3	
A	×	×	×	3
B	×	×	×	3
C	×	×	×	3
D	×	×		2
E	×		×	2
F		×	×	2
G	×	×		2
H	×		×	2
I	×	×		2
J	×			1
K		×		1
L			×	1
	9	8	7	

Answer-pattern

x	0	1	2	3	Sum
N_x	0	3	6	3	12
N_x^2	0	9	36	9	54
n_x	12	9	8	7	
$n_x - n_{x+1}$	3	1	1	7	
$\lvert n_x - n_{x+1} - N_x \rvert$	3	2	5	4	
$(n_x - n_{x+1} - N_x)^2$	9	4	25	16	54

$n_0 = 12.$

$n = 24/3 = 8.$

$h = 54/86 = 0{\cdot}63.$

The *a priori* probability of hig by the former formula is $1 - 0{\cdot}22 = 0{\cdot}78$. Taking the steepness as being roughly proportional to $n_1 - n_3$, this test had steepness 2. A further four similar examples, plus the above, yielded the following table:

Test	1	2	3	4	5
Relative steepness	9	5	4	2	0
A priori hig	0·10	0·59	0·63	0·78	0·89
Coefficient h	0	0·19	0·35	0·63	1·0

The positive correlation between *a priori* hig, the coefficient h, and the inverse of the steepness is obvious. Naturally the relations cannot be exact.

The conclusions so far arrived at may be conveniently summarized here before the experiment proper is described:

(i) The type of score-scatter, with little or no hig present, is determined by the shape of the answer-pattern.

(ii) The closeness with which the score-scatter and answer-pattern are related depends on the amount of hig present.

(iii) The amount of hig present depends on the steepness of the answer-pattern.

(iv) The amount of hig present can be measured.

These theoretical conclusions were tested in the experiment now to be described.

VII. THE EXPERIMENT.

As opposed to the usual school subject test, the typical group intelligence test conforms very well to the conditions for preordaining the score-scatter. It consists of a number of independent items ranging in difficulty from very easy to very difficult, *i.e.* it is steep. The injunction "Begin at the beginning and go straight through" also helps to minimize the amount of hig present. One such group intelligence test (Moray House Test 9) consisting of 94 items was chosen for the experiment. It had been independently standardized, and had also been used in two areas in England. For our present purpose a special preliminary trial on 30 children was made, and the answer-pattern proved to be steep as expected, the number of correct answers to each item ranging from 29 to zero. There were no interdependent items in this test.

The experiment proper consisted in designing, by choice from the items of the above test, three tests to give certain types of score-scatter, and then examining the answer-patterns and score-scatters actually obtained. The first test, test *A*, was to be designed to give maximum hig, and therefore maximum freedom to the score-scatter. Tests *B* and *C* were to be designed to produce score-scatters skewed positively and negatively respectively. For them it was therefore essential to have steep answer-patterns, and moreover answer-patterns of given shapes, so that their differences should provide the required skew curves.

Now Moray House Test 9 had been shown to be steep, and at the same time reliable data were procurable from which the answer-pattern could be obtained, viz. results from 800 children in an English borough. From this test therefore it was decided to make three tests *A*, *B*, and *C*, each of 15 items, corresponding to the above description.

The work of finding the answer-pattern was facilitated by the use of mechanical counters mounted on a stand. The 800 papers were treated 100 at a time, and after the second hundred had been finished it was found that the correlation between the answer-patterns of these first two hundreds was, by the Footrule, 0·997 with a P.E. of 0·0004, and therefore it was concluded that the 200 (really 202) papers formed a sufficient sample. The answer-pattern of these 202 papers was practically a straight line (see Fig. 1). From such an answer-pattern with a low degree of hig there should result a score-scatter consisting of a straight line parallel to the x-axis, *i.e.* every score should have about the same number of candidates. Now the degree of hig was very low ($h = 0{\cdot}043$); yet the score-scatter was the normal curve usually obtained in intelligence tests, so that the important conclusion can be drawn that *the normal or Gaussian character of the distribution of mental ages found with intelligence tests of the type of Moray House Test* 9 *is not caused by, but occurs rather in spite of, the nature of the answer-pattern coupled with a very low degree of hig.*

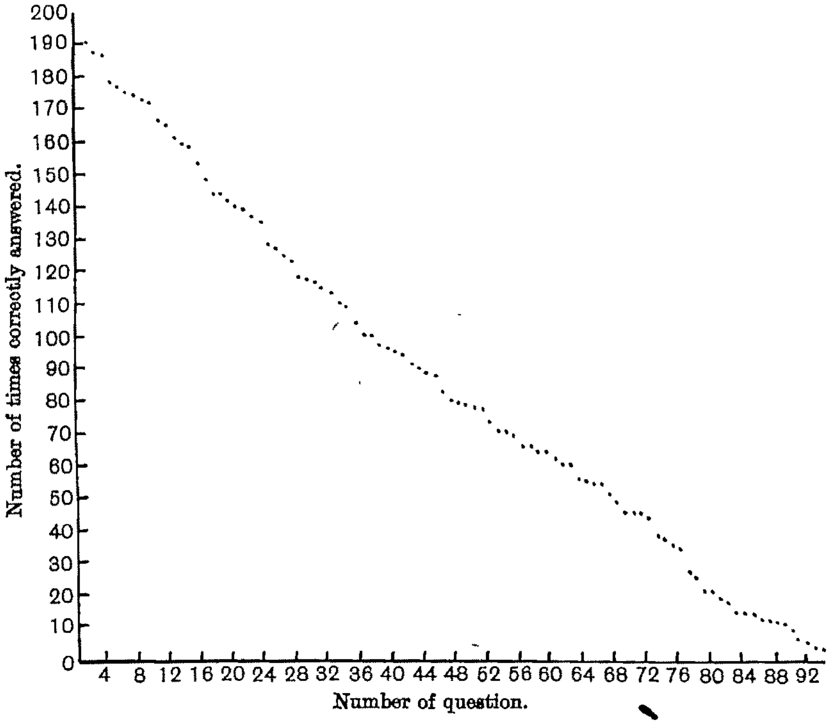

Fig. 1. Answer-pattern of Moray House Test 9. (202 candidates.)

From the above data the three tests A, B, and C were now constructed. Test A was designed to give a maximum amount of hig. Its 15 items were therefore chosen to be of as uniform difficulty as possible, a medium[1] degree of difficulty being chosen.

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[1] Medium, that is, for groups of the ability level of the former Moray House Test 9 sample. Unfortunately it did not prove medium for the group tested.

Test *B* was designed to give the minimum amount of hig, and a score-scatter skewed positively, *i.e.* with the tail to the right, or so as to give a median score of 5 and space out the candidates at the top. Its items therefore were chosen to increase rapidly in difficulty to question 10, then slowly to question 15. Test *C*, on the other hand, had items increasing slowly in difficulty up to question 5, and then quickly up to 15. The first items in tests *B* and *C* were of identical difficulty, and also the last items.

As additional aids in the control of hig, the directions in test *A*, where maximum hig is wanted, were all printed before each question, so putting each question on an equal footing in this respect. The candidates were also informed that all questions were equally easy, and that they might tackle them in any order. The questions were printed in descending order of difficulty, to equalize what little difficulty differences remained.

In tests *B* and *C*, the directions were only printed before the first question of each type, and also the injunction "Begin at the beginning and go straight through" was printed at the head.

The three tests, being short, could be given in a continuous test of half an hour's duration. To equalize time conditions, special directions were given to the supervisors, and practice and fatigue effects were avoided as far as possible by giving one-sixth of the candidates the tests in the order *ABC*, one-sixth in the order *ACB*, and so on for the other possible orders *BAC*, *CAB*, *BCA*, *CBA*.

Ninety-six children of the ages 10 and 11 in one school, and 70 in another, sat the tests. The results were treated separately, but only the results for the combined group are given here, except for coefficients of hig, tabulated separately.

VIII. Results.

Answer-patterns obtained. These are shown in Fig. 2. It will be seen that, while some items seem to have altered their positions slightly in the order of difficulty, the essential features of each test's answer-pattern have been preserved.

Hig. Test *A*, it will be remembered, was designed to give maximum, tests *B* and *C* minimum hig. The actual coefficients of hig obtained are shown in the table:

Test	*A*	*B*	*C*	
1st group data	0·35	0 09	0·045	96 cases
2nd group data	0·54	0·12	0·13	70 ,,
Total data	0·40	0·092	0·065	166 ,,

The disparity between h_A and h_B or h_C is obvious.

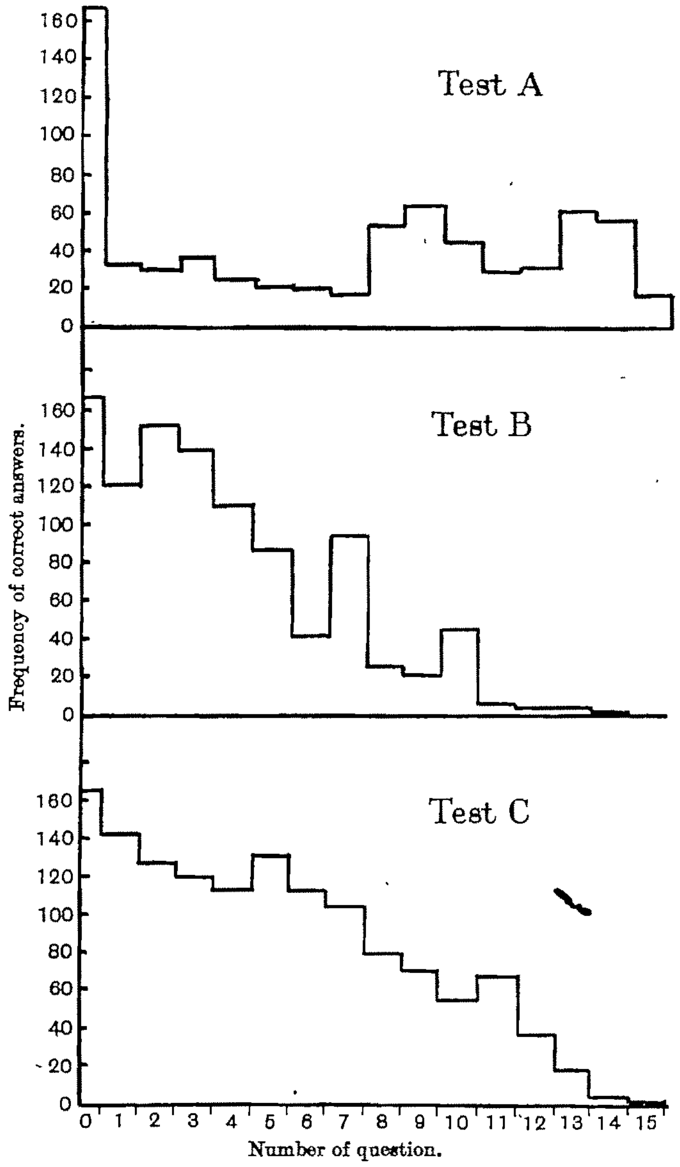

Fig. 2. Histograms of answer-patterns, *i.e.* number of times each question is correctly answered, obtained with the three tests A, B and C. (166 candidates.)

Score-scatter (see Fig. 3). The most noticeable feature, at first, is that test *A*, instead of giving the normal curve one would expect from the level nature of the answer-pattern, has yielded such a large number of zero scores. This is due to the ability level of the population tested being lower than that of the sample which sat Moray House Test 9. The

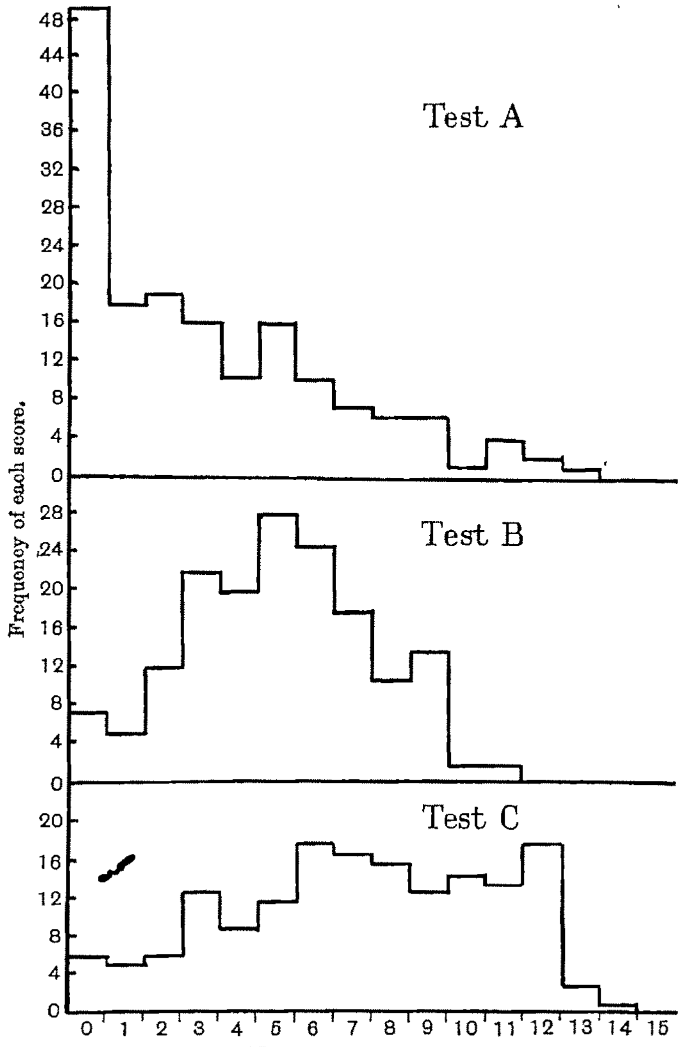

Fig. 3. Histograms of score-scatters obtained with the three tests A, B and C. (166 candidates.)

raising of the level of difficulty of test *A* will tend to skew the score-scatter positively, and this is what has happened.

Test *B* shows a median score of 5 as required and is skewed positively, so as to open out the candidates at the top. The formula

$$\text{skewness} = \frac{\mu_3}{\sigma^3},$$

where μ_3 = third moment and σ = standard deviation, gives here a value $S = 0{\cdot}01$, positive though slight.

Test *C*, which was designed to give a negative skewness, so as to space out the candidates at the bottom, does so very satisfactorily, having a coefficient of skewness of $-0{\cdot}24$. The effect is rather clouded in the total data, but the first set showed the separation of the poorer candidates very well. Of 96 candidates, 2 scored zero, 1 scored 1, 3 scored 2, 8 scored 3 and 2 scored 4, the other 80 candidates being in the region of scores 5 to 15.

Both the purposes for which the tests were designed have therefore been fulfilled, the amount of hig has been reduced to a very small amount, and the two tests (*B* and *C*) have yielded, from the same group of candidates, score-scatters skewed in opposite directions.

IX. CONCLUSIONS.

The following conclusions apply only to those tests which are composed of independent items. In the above paper they have been derived theoretically and tested practically.

1. The factors which predetermine the character of the score-scatter are the individual and relative difficulties of the questions, the graph or table of which has been called the answer-pattern. Each type of answer-pattern *tends* to produce its own type of score-scatter, the curve of the latter tending to be the differential of the curve of the former.

2. The factor which influences the extent of such predetermination is what has been called 'hig' (higgledypiggledyness of answering). Complete absence of hig leads to complete dependence of score-scatter on the answer-pattern. Experiment showed, however, that even a small amount of hig allows the 'normal' nature of score-scatters latitude to assert itself, so that with intelligence tests the normal distribution of mental ages found is not caused by, but occurs in spite of, the nature of the answer-pattern.

3. The incidence of hig depends on the steepness of the answer-pattern of the test, so that from the answer-pattern it is possible to pre-

dict not only the most probable type of score-scatter but also the degree of hig likely to be present, and therefore the accuracy of the prediction of the score-scatter.

4. An exact measure of the incidence of hig in any test whose results are known is provided by the coefficient h.

5. It is rather the differences of difficulty between the questions, than the absolute difficulty of the hardest, or the easiest, question, which determines whether a test will open out the top, or the bottom, candidates.

(*Manuscript received* 19 *November*, 1930.)

PUBLICATIONS RECENTLY RECEIVED.

Creative Mind. By C. SPEARMAN. London and Cambridge: Nisbet and Co. and The Cambridge University Press. 1930. Pp. xii + 153. 5*s.* net.

This book is the first of a new series planned and directed by Dr F. Aveling and intended to present "the problems of psychology in a popular way but at the same time without any loss of scientific accuracy." Prof. Spearman has succeeded in living up to this aim of the series in a remarkable manner, for his book is at once lively and interesting reading and a genuine and important contribution to its most difficult subject. In one sense it is an application of his well-known noegenetic point of view to a new field, but it is more than this. It abounds in acute and valuable observations and speculations on many topics relating to the constructive work of the mind. To many readers it may appear that the main principles used are descriptive rather than explanatory, and that when, as in the case of the quantitative principles, the standpoint of explanation is achieved, the general position becomes at once more highly speculative and hardly adequate to the facts. There seems no doubt whatever that all constructive mental work may be perfectly validly described as the eduction, or appreciation of relations, and correlates, but the critical reader will want to know what further conditions for them are essential besides the mere presence of the necessary data. It seems as if invention and discovery ought to be a great deal more common, and would be much more commonplace, than they actually are if they followed immediately upon their essential data as these are described by Prof. Spearman. That the occurrence of the processes required is determined solely by a quantitative factor of the amount of nervous or mental energy available appears to be a sweeping generalization for which the available evidence is not adequate.

Yet when all is said the book remains a noteworthy contribution, full of interesting illustrations most attractively set forth. It deserves wide reading and serious consideration. It is challenging, provocative of thought and experiment. It will interest the layman, intrigue the psychologist, and should give a real push forward to the tangled subject with which it deals.

Psychologies of 1930. Edited by CARL MURCHISON. International University Series. Clark University Press. Pp. xix + 497. $ 6.00.

This is partly a sequel to *Psychologies of* 1925, and partly an entirely new work. Most of the contributors to the former volume have also contributed to this one, but as was to be expected they have little that is new to say after so small an interval. The newcomers are Alfred Adler, E. G. Boring, G. S. Brett, Harvey Carr, John Dewey, J. C. Flugel, Pierre Janet, T. L. Kelley, K. N. Kornilow, J. P. Nage, I. P. Pavlov, F. Sander, A. L. Schniermann, C. Spearman, L. T. Troland, Margaret Washburn, and Albert Weiss. They certainly contribute to the rather bewildering variety of view which confronts the student of modern psychology. Actually the contributions are of very varying degrees of merit and of clarity, and some of the papers are of merit only in the light of contemporary history of psychology. For example, Kornilow's account of *Psychology in the Light of Dialectric Materialism* is nothing but a curiosity. However, the volume, together with its predecessor, certainly ought to be in every psychological library and should be widely used by all students who have arrived at a reasonable maturity of judgment.

Genetic Studies of Genius. Vol. III. Edited by LEWIS M. TERMAN. London: Geo. G. Harrap and Co. 1930. Pp. xiii + 508. 21*s.* net.

The third volume of this monumental research is called *The Promise of Youth* and describes a "follow up" investigation of one thousand particularly gifted children some ten to fifteen years after their original test assessments. The whole record, which is very well documented and clearly stated, is deeply interesting. It reveals a considerable and encouraging stability of unusual gifts with increasing age.

Taken together, the three volumes now published by Prof. Terman and his assistants are a most important and weighty contribution both to general psychology and to educational work and methods. They should have a permanent place as standard treatises in the realm with which they deal.

Sound and Meaning in English Poetry. By KATHARINE M. WILSON. London: Jonathan Cape. 1930. Pp. ix + 352. 10*s*. 6*d*. net.

What is the nature of the marriage between sound and meaning which constitutes poetry? The psychologist, as well as the prosodist, should contribute to this discussion, and the aesthetics of music as well as of literature is involved. It is fortunate that the author of this stimulating and provocative book has interests in all these camps.

Her argument may perhaps be summarized in this way. Music is probably more primitive than speech, which is an adaptation of music to serve utilitarian ends. Both poetry and prose bear the marks of this relationship in the possession of a music not merely in their words and rhythms, but in the inflections or modulations of the speaking voice. In prose these inflections, although they should be in some sort consistent, need not make a melody: in poetry they do, and this perhaps is the most fundamental difference between the two ways of speech.

The original poetry was presumably sung poetry: spoken poetry differs in that the pitch of the speaking voice moves continuously instead of jumping from note to note, but the essentials of song are still present, and we may—if profited thereby—transcribe the spoken poem into a musical notation of sorts. The work of Scripture (*Researches in Experimental Phonetics*) on the modulations of pitch in spoken words and sentences is relevant in this connection, and the author points the need for more studies along these lines, dealing with the English of England as well as with that of America.

The studies of Miller (*Science of Musical Sounds*) and Scripture (*op. cit.*) show that each vowel has its own characteristic pitch, although these investigators differ as to the relations which exist between this characteristic pitch and the pitch of the particular note to which the vowel may happen to be sung. Consonants tend to the class *noise* rather than *note*, and are more difficult to study, but they divide clearly into families, some mixing well and others badly, as Raymond (*Rhythm and Harmony in Poetry and Music*) among others has pointed out.

The *sounds* of music and of poetry differ in fact far less than might on first thoughts be supposed. Is there a more fundamental distinction in *meaning*? Poetry uses words with a dictionary reference, music does not; but in considering aesthetic meaning (broadly defined by the author as "the whole content of the mind as filled by the thing that gives meaning") this is not the major consideration. The problem of how words work in us is again one for psychology. The merely scientific reader may be blind to the imagery of "sweet peas on tip-toe for a flight": the merely musical reader may be sensitive only to rhyme and rhythm and cadence. Could we not have more psychological studies of the ways of experiencing poetry?

Lastly, how does meaning express itself in sound? Onomatopoesia is obviously fundamental, and takes us back to the origin of speech in vocal gesture. Sense and sound are not separates to be reconciled, and poetry is not compounded of two elements—music and meaning—but is one thing, meaningful sound. Rhyme and alliteration, for example, are not the mere ornaments of poetry: the poet's thought comes in rhymed form, as may be seen from a study of the assonances in the unrhymed verse of Campion, Milton, and many of the Elizabethans. The exquisite vowel music of Campion and Spenser, depending largely the one on echoes, the other on melodious sequences, is illustrated at some length in one of the most attractive sections of the book. Best of all perhaps is a final chapter of some fifty pages devoted to a detailed analysis of the *Ancient Mariner* as a classic example of the indissolubility of sense from sound.

Restriction of output among Unorganized Workers. By STANLEY B. MATHEWSON, with chapters by W. M. LEISERSON, H. S. DENNISON and A. E. MORGAN. New York: The Viking Press. 1931. Pp. xii + 212.

Restriction of output is frequently ascribed to the trade union movement. That this is not the whole truth is evident from this very interesting account of the state of affairs found by Mr Mathewson in works in America where the men were non-unionists. Mr Mathewson, it should be added, lived and worked with the men whose methods and attitudes he was studying. His experiences are amplified from those of six other men who were working in other industrial centres.

Mr Mathewson shows that restriction is practised by the non-union worker just as much as it is by the member of a trade union. It is practised for many reasons and in many ways. Fear of unemployment, laziness, personal grievances, etc., are important factors; but perhaps the most important is the war with the time-study man, as he is shown at work in this study.

Mr Mathewson makes various suggestions as to the way in which the situation may be alleviated. For these, and for the views of the three authorities who have contributed chapters on "The ceremonies of restriction": "What can employers do about it?" and "Conclusions," those interested in labour problems are referred to this book.

Psychopathology. A survey of modern approaches. By J. ERNEST NICOLI, with foreword by W. H. B. STODDART. London: Baillière, Tindall and Cox. 1930. Pp. xii + 203. 10*s.* 6*d.* net.

It is an unfortunate practice with many writers of books on psychology to insert, at the end of their main work, reprints of papers which have been published elsewhere. Dr Nicoli has followed this lead. Some *ninety-two* pages only of print are devoted to an outline of various views on psychopathology: the rest of this book consists of reprints of three papers. The first paper is said to be "an almost verbatim transcription of some impromptu remarks made before an informal discussion at a meeting of a local group of the Psychopathology Sub-Committee" on the concept of the ego in psychiatry; the second is a paper on type psychology, in which Jung's types are considered to the extent of 21 pages—the *whole* of Jung's psychology having been previously dealt with in under 12 pages—and in which the importance of a consideration of types, for example in the management of the staff of an institution, is stressed; the third paper is confined to a discussion of the Herd Instinct, and the conclusion is reached that "there is little need (in psychopathology) for a mere instinct of gregariousness and as to invoking any broad herd-system as a factor in mental derangement, it would be regarding this factor as much more unitary and definite than it really is, and the very comprehensiveness and complexity of constructions of such a system would make it too unwieldly for clinical explanations. Last, but not least, it would introduce still more complexity as those 'mental mechanisms' that are already too numerous and ill-defined." This may be so; but, if so, one fails to understand why almost half as many pages are devoted to the herd instinct as are allotted to the whole subject of psychopathology.

I criticize this feature at length because it is obvious that Dr Nicoli could have written an exceedingly valuable book. There is no doubt but that he knows his subject-matter, and a clear exposition of the various theories, with proper emphasis on their similarities and points of difference, would have been a great help to many students; but the outlines of the various theories are too condensed to be of use. Prince, Freud, Adler, Jung, Rivers, Watson, Kempf, Berman, Biochemical and Physiological Theories, Kritschmer and others cannot usefully be expounded in 28,000 words. Dr Nicoli has endeavoured to do this. He has succeeded better than most would have thought possible. One closes the book regretting that he had not doubled the length of each chapter—and omitted the three appendices.

Suggestion Therapy. By E. JOLOWICZ. *Hypnosis and Hypnotherapy.* By G. HEYER. Translated by ARNOLD EILOURT. London: C. W. Daniel and Co. 1931. Pp. 237. 8*s*. 6*d*. net.

Suggestion Therapy gives an account of several of the methods both of direct and of indirect suggestion as they have been developed and used in recent medical practices mainly in France and Germany. There is some discussion, not over clear, of the mechanisms through which these methods may be supposed to work. *Hypnosis and Hypnotherapy* gives an account in considerable detail of ways of inducing hypnosis and of the kinds of case to which this method of cure may be applied. Here again the theoretical portions lack clarity. The book is in the main addressed to the medical practitioner, and will be found useful both by him and by the instructed layman. The authors are duly cautious in their recommendations. The translation is of the literal rather than of the literary kind.

Analyse der Suggestivephänomene und Theorie der Suggestion. By A. HERZBERG. Berlin: S. Korger. 1930. S. 128 M. 7.

The first part of this interesting monograph deals with various points of view from which the phenomena of suggestion and related phenomena have been studied. This is clear but not very comprehensive. The second part attempts an original analysis, based upon actual observation, of these phenomena in their individual and social aspects. The third part discusses the theory of the matter. A rather brief bibliography is appended.

Aphasia in Children. By ALEX. W. G. EWING. Oxford Medical Publications. Oxford University Press. 1930. Pp. xi + 152.

This book contains observations of an extraordinary interest and theoretic value. Briefly, Ewing has found that a number of children never learn to speak or understand speech, and may even be thought to be deaf mutes, who suffer only from a relative deafness for sounds of high frequency. These sounds appear in a number of vowels and consonants, so that without them the vowels and consonants cannot be discriminated and heard speech is therefore meaningless. With ingenious tests Ewing discovers that the supposed deaf mute can hear and then trains the perception for high frequency sounds by using great intensities. Eventually the children are thus taught both to hear words and to use them. He also examined a few cases of acquired aphasia and found the symptoms different from those in congenital aphasia of his type. From this he concludes that the two conditions are fundamentally different—a conclusion by no means inevitable and not substantiated by evidence of any extensive knowledge by the author of acquired aphasia. The book suffers from being written in a jerky style and it is difficult to get a clear clinical picture of any one case, for in no instance are the history, symptoms, treatment and outcome given seriatim. All the data are scattered throughout the book and the reader has to do such synthesis as he can.

L'Évolution de la Mémoire et de la Notion du Temps. By PIERRE JANET. Paris: A. Chahine. 1928. 55 fr.

Of these three volumes the first deals with *La Durée*, the second with *La Mémoire Élémentaire* and the third with *L'Organisation du Temps*. They consist of lectures given at the *Collège de France* and are published practically as they were spoken. They are a brilliant and notable contribution to their subject-matter, abounding in the most acute psychological observation, and full of delightful humour. The general position is that man, faced by a threatening world, has been forced to discover or invent progressively more and more adequate ways of dealing with his practical difficulties. The most significant of these concern ways of learning to utilize absent objects and along this path has developed all memory reactions and all notion of time. Typically the situations that give rise to these developments are social situations. Every serious psychologist would enjoy and profit from a careful reading of these most attractive discussions.

Social Psychology. By E. T. Krueger and W. C. Reckless. London: Longmans, Green and Co. 1931. Pp. vii + 578. 15*s*. net.

This is intended definitely as a student's textbook. Each chapter has its bibliography and its exercises and questions. There is no doubt that a good introductory study of social psychology is still very much needed, but this book does not meet the need. There is no clear emergence of any well-defined point of view. Much of what is discussed might just as well be considered in general individual psychology and some of the rest belongs properly to sociology. Nothing stands out clearly and even the references to other work are overweighed by the desire to be very comprehensive. Altogether the book is disappointing.

A Changing Psychology in Social Case Work. By Virginia P. Robinson. University of Carolina Press. (London: Humphrey Milford.) 1930. Pp. xvii + 204. 11*s*. 6*d*. net:

This book contains two parts. Part I contains a condensed but accurate and clear presentation of the development of individual psychological studies prior to 1920. The stress here is laid mainly upon the recognition that the dominant notion in studying and developing social adaptability is that of understanding the individual person. Part II is called "The Emergence of Relationship." Some of this again is historical and gives a clear and just account of the growing recognition that no individual can be properly understood except by a careful study of his social setting and his relationships to other individuals. Some of it is constructive and shows how this wider conception may be clarified and applied. The whole book is well worthy of study. It contains numerous illustrations well chosen and treated, and it is written in a sober and unexaggerated strain. There is a selected bibliography which will be useful, though, perhaps, the selection might have been a little more drastic.

The Primary School. Report of the Consultative Committee of the Board of Education. London: H.M. Stationery Office, 1930. Pp. xxix + 290. 2*s*. 6*d*. net.

The publication of this Report marks a definite step in the advance of educational psychology. Not so much by way of the addition of new knowledge—there is nothing new to psychologists in it; but by way of the complete transformation of educational values. For the first time in history, the aims and technique of education for children between seven and eleven years are conceived as necessarily rooted in the facts of their growth and development. Every part of the discussion of curriculum and methods shows the fertilizing influence of the notion that the work of the Primary School "must be to supply children between the ages of seven and eleven with what is essential to their healthy growth—physical, intellectual and moral—during that particular stage of their development" (p. 92). And the wisdom of basing the internal organization of the school on a classification of the children by their capacities and natural gifts is fully accepted.

The clear and well-written chapter surveying the broad facts of mental development is chiefly based upon Prof. Burt's memorandum, itself summarizing what is known about the various aspects of mental life in these years, and included as an appendix. Another very valuable memorandum on anatomical and physiological development, by Prof. H. A. Harris, is appended.

Prof. Burt, after drawing attention to the great field for research still offered by these years of psychological growth, deals first with what is known of intellectual development. He uses the close inter-correlations of specific abilities in these years and the uniformity of growth in general intelligence, to set aside the older 'stratigraphical' and 'recapitulatory' theories of development. The characteristics of the various sensory, motor and 'higher' mental capacities and operations are summarized, and the general implications of all these facts brought out most admirably.

He then raises some of the many questions still at issue with regard to emotional development and instincts, where he deals rather more tenderly with the 'recapitulatory' and 'culture-epoch' theories. But the direct descriptive facts of the spontaneous interests of children from seven to eleven, as shown in their play, their drawing, their favourite handicrafts and their reading tastes, which Prof. Burt is able to offer, suggest how gratuitous such 'theories' are.

Introduction to Psycho-analysis for Teachers. Four Lectures by ANNA FREUD. Translated by BARBARA LOW. London: George Allen and Unwin, Ltd. 1931. Pp. 117. 3*s.* 6*d.* net.

The four lectures deal with the following topics: Infantile amnesia and the Oedipus Complex; The infantile Instinct-Life; The Latency Period; The relation between Psycho-analysis and Pedagogy. They contain little or nothing that is new, but they are exceedingly clear, and present the psycho-analytic position concerning the matters discussed with considerable force. Miss Barbara Low has done her work as translator in an excellent fashion.

Sin and the New Psychology. By CLIFFORD E. BARBOUR. London: George Allen and Unwin. 1931. Pp. 224. 6*s.* net.

The purpose of this book is to reconcile Christianity and psycho-analysis. All that is true in the latter—and the author is inclined to consider that there is much that is true—had been already formulated and proclaimed in the former. It is probable that few psycho-analysts, and perhaps still fewer theologians will be willing to admit this. However, in the course of his advocacy of this view Dr Barbour draws many interesting parallels. His writing is clear and he is well informed as to his problems.

An Introduction to the Psychology of the Class Room. By C. E. HOLLEY. Boston: D. C. Heath and Co. 1930. Pp. x + 257.

This is planned as a textbook for intending teachers, and is written with a view to avoiding all those technical parts of psychology which the average teacher finds it difficult or useless to try to assimilate. The result is clear enough but undistinguished and in no way original.

La Méthode Concentrique dans l'étude des psychoneuroses. Par M. LAIGNEL-LAVASTINE. Paris: A. Chahine. 1928. Pp. 279.

The Concentric Method in the Diagnosis of Psychoneurotics. By M. LAIGNEL-LAVASTINE. London: Kegan Paul, Trench, Trübner and Co., Ltd. International Library of Psychology, etc., 1931. Pp. x + 217. 10*s.* 6*d.* net.

These are a series of lectures delivered at La Pitié Hospital in Paris. They are very much better in French than in their English form, for this translation into English is extremely wooden. The concentric method in diagnosis seems to consist mainly in a classification of different points of view (called here 'zones') from which the psychoneurotic may be examined or studied. It would be unfair to the author to attempt to state very briefly his method, for such force as it has can become apparent only by a careful study of his illustrative cases. On the whole the most interesting of the lectures deals with the relation of psychoneuroses to various ascetic and mystical experiences. Here in particular some excellent illustrative material has been collected.

The various plates which adorn the French volume have not been reproduced in the English.

The Pattern of Life. By ALFRED ADLER: edited by W. B. WOLFE. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. 271. 8*s.* 6*d.* net.

Dr Wolfe begins the book by an appreciative essay on the work of Alfred Adler. The rest of the volume consists of popular lectures, each discourse being a case study of some 'difficult' child. Nothing new comes out of Adler's remarks about these selected cases. The whole thing is a bit of very easy book-making.

The Education of Children. By Alfred Adler. Translated by Eleanore and Friedrich Jensen, M. D. London: George Allen and Unwin, Ltd. 1930. Pp. iii + 309. 12*s.* 6*d.* net.

There is little that is new in this book. The author stresses the importance of right adult guidance in the mental and social growth of children, he outlines the general form and nature of the normal young individual's struggle for development and expression, and quotes instances of the kind of problems to which children are from time to time forced to adjust themselves.

An account is given of the particular influences likely to affect and trouble the child at school, and methods are suggested for handling or avoiding these. In an appendix there is an Individual Psychological questionnaire, together with five case histories and commentaries.

Emotions of Men. By F. H. Lund. London: McGraw House Publishing Co., Ltd. 1930. Pp. x + 348. 12*s.* 6*d.* net.

Emotions of Men is a pleasantly produced, lively and popularly written book. It deals with a variety of problems of emotion. It is as accurate as could be expected, though it indulges in a number of generalizations and is often over-dogmatic in its statements. The two chapters on *Emotions in History* and *Emotions in Politics* provide some interesting material and cases for the social psychologist. The study is well calculated to attract the psychologist in his lighter hours and ought to have a considerable appeal to the general reader.

Religion and Life. By W. B. Selbie. Oxford University Press. 1930. Pp. v + 135. 6*s.* net.

Dr Selbie's new book consists of six lectures delivered in the University of Harvard under the William Bolden Noble Foundation. Their topics are: Religion and History; The Psychological Interpretation of Religion; Belief in God; Religion and Ethics; The Christian Contribution; and Eternal Life. Only the second lecture specifically concerns psychology. In this Dr Selbie welcomes the contribution made by psychological study towards an understanding of religious faith and life, and maintains that one of its main issues is that it helps to show how religion may unify and deepen the significance of human experiences. The lectures are a characteristically sane and balanced contribution to their subject.

Philosophy of the Unconscious. By Eduard von Hartmann. Translated by C. K. Ogden. London: Kegan Paul, Trench, Trübner and Co., Ltd. International Library of Psychology, etc. 1931. Pp. xxxviii + 368. 15*s.* net.

This new translation of von Hartmann deserves a warm welcome at a time when public interest in views about unconscious mental processes has been violently stimulated by psychological research and speculation. Mr Ogden has done his work very well indeed, and the present edition is far more handy and useful than any other for the English student.

Organische Erziehung. By R. Drinkuth. Berlin: Otto Elsner. Pp. xii + 66. 4,40 R.-M.

In this essay the author applies the Marburg 'structural' psychology of types to education. The first part is concerned with a general discussion of method. The psychology of types tries to show that there are laws operative within individual differences, and that each individual is bound by his type to interpret experience in a certain way. Each type, too, has its value, and so far from attempting to force everyone into the same mould, which educational systems are inclined to do, they ought to take account of these general differences of type and try to preserve the most valuable aspects of each.

The second part gives a concise and clear summary of the various types that have been elucidated by the Marburg school such as the integrate and its subdivisions (I_1, I_2, I_3) the synaesthetic, and the disintegrate. But some of the general remarks have a tendency towards over-generalization: "The same applies to the difference between youth and age in the history of civilization; the old cultures evince a strong integration."

The last part deals with integration and disintegration (in the Marburg sense) in its relation to the phases of human development. The author uses Charlotte Bühler's division into childhood, prepuberty, puberty, and adolescence, and shows that in each of these phases there is a struggle between the fundamental 'structural plans' of integration and disintegration, one of which finally becomes the permanent aspect of the personality. It is during the second and third phases that educational method should adapt itself to the fluctuating mode of integration. When the child is in an integrate state, it should not be forced to behave in a disintegrate manner, though it is necessary that the integrate adolescent should learn discipline and continuous endeavour, which are disliked by the integrate.

Die Theologie Calvins. Ihre innere Systematik im Lichte strukturpsychologischer Forschungsmethode. By H. WEBER. Berlin: Otto Elsner. Pp. 68. 4,40 R.-M.

The author approaches the relation between theology and psychology from the typological, structural side. An analysis of Calvin's type of mind is made, using the results of the Marburg school, and this throws a new light on the important principles of his theology. The method is of interest to psychologists perhaps more than to theologians; but it cannot be ignored by either.

The following eleven volumes are all published by Beyer und Mann, Langensalza. They form part of a large collection entitled *Friedrich Mann's* PADAGOGISCHES MAGAZIN, *Abhandlungen vom Gebiete der Pädagogik und ihrer Hilfswissenschaften.* It already contains some 1350 numbers, which range over all the practical and the theoretical aspects of education, psychology, and philosophy. Its aim is to provide literature of this kind in a compact form at a moderate price; its success can be gauged from the fact that many volumes have run through several editions. It contains treatises by Ziehen, Jaensch, Kroh, Becher, Vaihinger, Tumlirz, and numerous other distinguished psychologists and paedagogues.

(1) *Die Gefühlsbetonung von Farben und Farbenkombinationen bei Kindern.* By ANNA MARTIN. 1921. Pp. 44. 0,75 R.-M.

An examination of the factors on which the emotional effect of colours depends in children. Simple associations often arise and influence the liking or disliking of a colour. Red has the most associations in both sexes, but girls on the whole prefer red to blue, whereas boys prefer blue to red. The investigation is of interest in connection with the training of colour discrimination.

(2) *Das Bild des Erziehers im Zogling.* By GJUEN LIU. 1931. Pp. 86. 2,20 R.-M.

One of the commonest and most difficult problems of education is what the author calls 'the contrary emotional attitudes' of the child towards the teacher, hate, fear, disrespect, etc. He gives reasons for their development and traces their intensity, the length of time they last, and their influence on the personality of the child, going on to discuss the paedagogical implications and the kind of attitude that is desirable.

(3) *Ein Beitrag zur Lehre von der Entwicklung der optischen Raumauffassung und des optischen Raumgedächtnisses bei Schulkindern.* By KARL ASMUS. 1922. Pp. 61. 1,50 R.-M.

Various geometrical figures were shown to children for eight seconds. They then had to reproduce them, and again after thirty minutes. In this way an examination was made of the powers of retention, synthesis, comparison, and abstraction. Following Hicks and Rivers, the author shows that eye movements have nothing to do with the over-estimation of small areas or the under-estimation of larger ones. He discusses the *Gestalt* problem, mirror images, and the kinaesthetic sensations that arise when straight and crooked lines, curves, etc. are presented.

(4) *Moglichkeiten und Grenzen der experimentellen Pädagogik.* By Prof. G. DEUCHLER. 1926. Pp. 34. 0,80 R.-M.

A brief but clear historical introduction is followed by an acute analysis of the fields of various branches of child and applied psychology, the formulation of their guiding principles, and a discussion of the biological, historical and sociological limitations of experimental paedagogy.

(5) *Das Erziehungsziel als Ausdruck sozialen Lebens.* By JOSEF SCHMIDT-HARTEFELD. Pp. x + 180. 1931. 5,20 R.-M.

The first part of this book attempts to bring some clarity into the many and conflicting definitions of sociology, its aim and scope. The second traces the development of sociological concepts in recent times, of various current ideals of education, and of the theories of values that underlie them. The author's discussions of the influence of Comte's positivism and the socialism of Marx and Engels on the formulation of aims and methods in education are very interesting and are illuminated by a wealth of illustrative quotation from an exhaustive bibliography. The last part contains a general philosophical critique of sociological systems, theories of values, and educational aims.

(6) *Eidetik und Schulunterricht.* By MARGARETE ZIMMERMANN. 1931. Pp. 88. 2,40 R.-M.

This is quite one of the most useful books on eidetic imagery that has yet appeared. It gives, in a short clear form, all the facts that have up to the present been assembled about this very important branch of child and general psychology and a full bibliography; and it does not hesitate to point out where the facts are insufficient to warrant the very wide generalizations that have sometimes been made. Thus, for instance, the theory of the 'unitary type' is considered 'not proven.' The last chapter deals with the special importance of the eidetic disposition for the schoolchild. For anyone who is about to embark on eidetic investigations, this should prove an invaluable 'pocket reference book.'

(7) *Analyse der freien Märchenproduktion.* By REINHARD NOLTE. 1931. Pp. 144. 3,60 R.-M.

This is an interesting analysis of the spontaneous production of fairy tales by the children on the Marburg South school. The children were shown a picture, and then proceeded to unfold and describe the images and tales that arose in their minds. The most vivid and original fantasias were produced by eidetic children belonging to the so-called B-type. The experimental part, on the influence of music and of differently coloured backgrounds on the course of the stories told by the child, throws interesting light on the emotional concomitants of fairy tales. The author surmises that primitive tales are largely eidetic in origin, and quotes a number of anthropologists like Lévy-Bruhl and Frobenius in confirmation.

(8) *Probleme der Charakterologie.* By OTTO TUMLIRZ. 1928. Pp. 69. 1,70 R.-M.

This essay reviews the characterologies and types of Rutz, Kretschmer, Ewald, Wahle, Alfred Adler, Kunkel, Klages, Spranger, and Utitz accurately and fairly. In each case the limitations and the reasons for them are indicated. The author draws general parallels between the various systems, shows the directions which research into character ought to take, and indicates its importance for education.

(9) *Die Grundlagen der Charakterologie.* Fifteen lectures by Prof. TH. ZIEHEN. 1930. Pp. viii + 362. 9,00 R.-M.

After the majority of 'psychobiographies' and pseudo-scientific works on the psychology of character this book comes like a blast of fresh air after fog. It is written by an author equally distinguished as a psychologist, a psychiatrist, a paedagogue and a philosopher, with that clarity which is the result of very wide practical experience and profound scientific insight. The book traces the causal factors in the development of character, the endogenous and hereditary, and the exogenous or environmental. The anatomical and physiological bases of character and temperament are fully dealt with as well as their development from childhood to senility. Too much space seems at first sight to be devoted to psychopathological manifestations, but that is chiefly due to the fact that in them normal aspects are seen 'in high relief' as it were, and that they have been studied far more fully and recorded more scientifically, without a value bias. The last sections deal with the prognosis and prophylaxis of character and temperament. In an appendix Ziehen has collected a number of characterological examples drawn from his practice in therapeutical paedagogy and criminal psychology.

(10) *Das Lebensrätsel des Instinktiven.* Vol. I. *Instinktforschung und Teleologieproblem.* By PAUL LUCHTENBERG. 1925. Pp. 107. 2,00 R.-M.

This is the first volume of a larger work, which deals with instinct as the central problem of the philosophy of biology. It is concerned with the various teleological theories of instinct that have been propounded by Driesch and others.

(11) *Über das Verhalten der Primitiven zum Tode.* By FELIX SCHERKE. 1923. Pp. 232, with a bibliography and an index of less known tribes. 3,50 R.-M.

The author surveys an extensive literature, from which he elucidates the main aspects of primitive ideas about the dead, and the relation of body and soul. He finds that the animistic stage is preceded by 'animatistic' and magical conceptions. The living take up an entirely egoistical attitude towards the dead, although at an early stage there are traces of the more civilized, non-egoistic attitude.

REGISTER OF THE BRITISH PSYCHOLOGICAL SOCIETY.

52 Upper Bedford Place,
W.C. 1.
May 1*st*, 1931.

Dear Sir,

May I through the *Journal* thank members of the British Psychological Society who have sent in registration forms to the Temporary Registrar? The trouble taken by members in making out these returns has been greatly appreciated by the Council.

The register, which has taken the form of a card index, is now nearing completion. It is hoped that those members who have thought the form too elaborate or too specialized for recording their connection with the Society, will not refrain on this account from sending in a return. Every form that shows a member's occupation and line of interest in psychology is of value for the register.

Yours faithfully,

Beatrice Edgell,
President.

To The Editor
British Journal of Psychology.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

March 14, 1931.	"A Brief restatement of the Case for Psychological Hedonism and an Examination of some Fundamental Concepts in Affective Psychology." By Mr Russell Sedgwick.

SECTIONAL MEETINGS.

Medical.

February 25, 1931.	"On Defining Psycho-analysis." By Dr Adrian Stephen.
March 25, 1931.	"A New Approach to the Problem of Psychoneurosis in Childhood." By Dr M. F. J. Lowenfeld.
April 22, 1931.	"A Clinical Study of Obsession." By Dr H. V. Dicks.
May 27, 1931.	"The Remote Consequences of Psycho-Analysis on Individual, Social and Instinctive Behaviour." By R. Money Kyrle, Ph.D.

Education.

March 2, 1931.	"Some Contrasted Aspects of Psycho-Analysis and Education," read by Miss M. N. Searl.
May 4, 1931.	"Four Cases Illustrating the Technique of Remedial Teaching," read by Miss M. MacTaggart, Ph.D.

Industrial.

March 26, 1931.	"The Idea of 'the thing as a whole' in Certain Forms of Learning." By M. Drury Smith, M.A., Ph.D. (of the Cambridge Psychological Laboratory).
April 16, 1931.	"A New Performance Test of Intelligence." By W. P. Alexander, M.A., B.Sc. (of the Department of Education, Glasgow University).

Aesthetics.

February 13, 1931.	"Reality in Life and Literature." By Mr R. Ellis Roberts.
March 12, 1931.	A discussion on her article, "Archetypal Patterns in Tragic Poetry," by Miss M. Bodkin, M.A. (*British Journal of Psychology*, October, 1930, p. 183.)
May 8, 1931.	A Paper with Musical Illustrations, given by Professor Donald Tovey.

JOINT MEETINGS.

(THE BRITISH PSYCHOLOGICAL SOCIETY AND THE CAMBRIDGE PSYCHOLOGICAL SOCIETY.)

February 14, 1931. A Discussion on "Neural Mechanisms in Behaviour," opened by Professor W. S. HUNTER, President Elect of the American Psychological Association.

A Paper on "Reflex and other Types of Response," by Dr R. S. CREED, of The Oxford Physiological Laboratory.

A Discussion on "Psychical and Physical Causation," by Dr C. D. BROAD.

Experimental Demonstrations were given on:

"Investigation of Temperature Sensations." By Dr J. T. MACCURDY.

"The Abstraction of Colour and Form." By Dr O. A. OESER.

"Class Demonstrations of Weber's Law for Vision and Hearing, Dark Adaptation, and the Variation of Acuity with Illumination." By Dr H. BANISTER.

"The Learning of Games of Skill." By Mrs M. DRURY SMITH.

"Peripheral Perception of Movement." By Miss M. D. VERNON.

"The Learning of Simple Movements in the Guinea-Pig." Mr G. C. GRINDLEY.

"The Effect of Glare on Perception." By Mr A. H. D. TOZER.

"Peripheral Perception of Form and Position." By Mr J. M. BLACKBURN.

"Electrical Responses from Skin Receptors in the Frog." By Dr H. HOAGLAND.

BRITISH PSYCHOLOGICAL SOCIETY

NAMES OF OFFICERS AND MEMBERS, 1931[1]

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology.*

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

10 *a*. Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express dis-

approval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, one representative of the Committee for Research in Education to be nominated by that Committee, and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two Ordinary Members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the Secretary at least two weeks before the Annual General Meeting.

24 *a. Nominations of representatives of the Sections, the Branches and the Committee for Research in Education shall be sent to the General Secretary at least two weeks before the Annual General Meeting.*

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of

the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council *and Committees* or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the Ordinary Members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

34 *a*. Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £3. 2*s*.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This

additional fee shall entitle Members of any Section to receive such additional, and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or, failing him, a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

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THE INFLUENCE OF INDIRECT INCENTIVES UPON THE ACCURACY OF SKILLED MOVEMENTS[1].

BY C. A. MACE.

(*From the St Andrews Psychological Laboratory.*)

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  - (2) *The optimum objective* (pp. 113–114).

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[1] The experiments here reported are an excerpt from a series of investigations in progress at St Andrews on behalf of the Industrial Health Research Board on the general subject of the influence of incentives upon efficiency in mental and physical work. The investigations are under the general direction of the author of the paper, but are wholly dependent upon the co-operation of a group of research students. The experiments referred to here have been carried out by J. M. Milne, J. W. Taylor and A. S. Robertson, to whose careful and untiring work scant justice is done by this very condensed report, prepared in the first instance for the purpose of a short communication to the Psychological Section of the British Association, London, 1931.

I. THE PROBLEM.

(1) *Indirect incentives.*

As an approximate generalization, subject to many reservations, it would appear that the utility of such obvious incentives as rewards and penalties varies inversely with the rank of the ability required in the task to be performed. Though it be that "every man has his price," the energies of bishops and cabinet ministers would seem to be less amenable to this species of control than are the energies of men of baser clay. Performance benefits from the ordinary incentives more obviously in tests of sensori-motor capacity than in tests of intelligence; and bonus payment systems have been less generally applied to the highly skilled worker than to the relatively unskilled.

If this be broadly true, various explanations are at hand. One relevant consideration is the fact that highly skilled performance requires a clear and a cool head, and concentrated attention. The cruder incentives may induce feverish activity and distract attention from the central conditions of the task. There is, in fact, experimental evidence that rewards and penalties may be detrimental to fine adjustments and to precision in co-ordinated movement.

The performance of any task is controlled by some specific intention—to strike the nail on the head, to complete in a given time so many units of industrial output, or to memorize so many units of erudition for a final examination. To introduce an extraneous penalty or reward evokes a secondary intention—to secure the reward or to escape the penalty. The influence of the secondary purpose upon the primary intention is sometimes beneficial, sometimes the reverse. It would seem to be frequently detrimental in the case of skilled performance, more particularly before full mastery of the operation has been acquired. The required incentives here should be in some way intrinsic to the task. They should not intervene in the form of a distinct, and perhaps conflicting, purpose. To stimulate endeavour, what is required is the operation of factors which will modify favourably the primary and central intention without creating secondary objectives. Factors which operate in this way will be called 'indirect' incentives[1].

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[1] 'Indirect' because their employment might appear to be a somewhat *roundabout* procedure as contrasted with the simple expedient of imposing a penalty or offering a reward.

(2) *Implicit standards.*

Among the most obvious factors influencing efficiency in almost any operation is the worker's conception of what constitutes a 'good' or 'reasonably good' performance—his conception of a 'good shot,' a 'fair day's work,' an 'adequate preparation' for his examination. Such conceptions may take the form of fully conscious opinions, but more frequently they are implicit. The notion of an 'implicit standard' does not involve any theory concerning 'unconscious ideas' or unconscious wishes or intentions. We may speak of an implicit standard when there is a level of attainment, performance at or above which results in experienced satisfaction, and when there occurs a pang of disappointment, however mild, if this level is not attained. By reference to the principle of 'trial and error' learning implicit standards might also be defined, if we so desire, in purely 'behaviouristic' terms. On this view an implicit standard would be operative when there is a level of attainment such that performance below this level is followed by continued 'trial and error' variation, and also such that the attainment of this level is followed by stabilization (provided stabilization is not accounted for by any 'physiological limit')[1].

The implicit standard, the central point in the neutral zone between the limen of disappointment and the limen of satisfaction, or the point of transition from variability to stabilization (presuming that these descriptions apply to the same phenomenon) is influenced by a number of distinct external conditions, and is in consequence subject to experimental control. In the course of experimentation the following devices have been employed: communication of exact knowledge of prior performance, publishing the averages of co-workers, artificial scoring systems, and various modifications of the intrinsic conditions of the task—the last-mentioned being the principle employed in the experiments here to be reported.

It was observed in the course of prior studies that subjects aiming at targets defined to themselves a 'good,' 'fair' or 'poor' shot not in terms of its *absolute* distance from the bull's-eye but in a way which was *relative to the form of the target employed.* Thus a satisfactory shot would be defined in terms of the units provided by the concentric rings of the target, without precise regard to their size. It would follow that a modification in the design of the target would entail a corresponding modification in the subject's standard of performance. Such a modification constitutes an indirect incentive. The following experiments were performed to verify the hypothesis that efficiency does in fact depend upon such implicit standards, and to determine more precisely the extent to which a modification of standard would influence the efficiency in performance. The subjects were university students. They were not informed as to the purpose of the investigation, but understood vaguely that the investigation was concerned with the form of the practice curve.

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[1] Unfortunately it would follow from this definition that any plateau is the expression of an implicit standard. Plateaus, perhaps, are commonly produced in this way, but it cannot be proved by definition.

II. THE EXPERIMENTS.

(1) *Muscio's aiming test.*

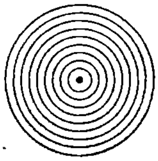
In the first series of experiments the test operation was that of Muscio's aiming test[1]. The subject spears with a pencil at a series of small targets printed on quarto sheets and attached to adjustable frames at arm's length before him. The efficiency of the operation was recorded in terms of the distance from the bull's-eye of the point of the target struck.

Targets of three types were used:

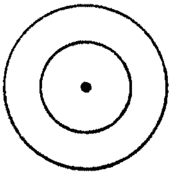
Trial Target, consisting of a bull's-eye 1 mm. in diameter and nine concentric rings at millimetre intervals from the circumference of the bull's-eye.

Target A, consisting of a bull's-eye 1 mm. in diameter, an inner ring 5 mm. from the circumference of the bull's-eye, and an outer ring 10 mm. from the circumference of the bull's-eye.

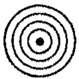
Target B, consisting of a bull's-eye 1 mm. in diameter, and four concentric rings at millimetre intervals from the circumference of the bull's-eye.



Trial Target.



Target *A*.



Target *B*.

The distribution of the targets on each sheet was identical with that of Muscio's forms. There was no guiding line to indicate the order in which the targets were to be struck, the subjects being familiarized with the routine by means of two full practice periods.

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[1] *Reports of the Industrial Fatigue Research Board*, No. 19, pp. 18–36.

Ten subjects were employed, each performing the standard task twice a day throughout five days. On the first day the trial target was employed for all subjects. On the basis of their performance they were then divided into two groups, A and B, equal in number and initial ability. On the four succeeding days Group A performed the test with Target *A*, and Group B performed it with Target *B*.

Experiments on an allied problem provided a comparable group of four subjects who performed the same series of tests with the Trial Target throughout. In this way the practical equivalence of Target *A* to the Trial Target was established.

The 'standard task' was as follows:

Three rounds of five sets per round (a 'set' consisting of ten shots) were performed at the subject's natural rate. The time taken for each set was recorded by a stop watch.

Three rounds of five sets were then performed at a rate controlled by the beat of a metronome. The metronome was set at 184 beats a minute. The subject speared at the target on one beat and withdrew his hand to the shoulder at the next, thus completing the cycle of the unit operation in 0·65 sec. The controlled rate of work was thus 6·5 sec. per set of ten shots. The task thus comprised 300 shots.

(2) *The dart-board tests.*

The second series of experiments fall into two parts:

(i) In the first a single dart board was employed, 10 in. in radius with circles marked in inches. Twelve subjects were employed. The test extended over six days. On the first day all subjects performed the standard task (throwing 100 darts, *i.e.* ten sets of ten throws) from a distance of 3·75 yd. On the basis of their performance they were divided into two groups very nearly equal in ability. On the succeeding four days subjects performed the test twice a day, members of Group A at a distance of 2·5 yd., those of Group B at a distance of 5 yd., from the target. (These conditions may be presumed to impose modifications in the implicit standard analogous to those imposed by variations in the size of the target.) On the sixth day each subject was re-tested at the trial distance of 3·75 yd.

(ii) In the second part of the second series of tests, hereafter called the Subsidiary Experiment, two dart boards were used; *A*, 10 in. in radius; *B*, 5 in. in radius. Each was subdivided by ten concentric rings. Five subjects performed the standard task (fifty shots at Target *A* and fifty shots at Target *B*) twice daily for four consecutive days. Darts were thrown from a constant distance of 2·5 yd.

III. THE FACTS.

(1) *Results of the Muscio aiming test.*

As noted above, the equivalence of Target *A* to the Trial Target was established by a comparable group of four subjects who performed the complete series of tests with the Trial Target. So far as any difference was observable, it was in favour of Target *A*. In the two practice periods the average errors of the Trial Target group for the natural rate of aiming were 4·68 mm. and 4·65 mm. The corresponding figures for the eight test periods were 4·44, 4·29, 3·98, 4·33, 4·01, 4·33, 4·22 and 3·95 mm. Comparing these figures with those shown in Table I, we find that this group, though of slightly higher initial capacity than Groups A and B, improved less with practice. It was equal in ability to Group A at the last test period. The results for tests at controlled rate are similar, performance with Target *A* being somewhat superior to performance with the Trial Target. In what follows we may therefore treat Target *A* as certainly not less favourable to accuracy of aim than the Trial Target, and restrict the comparison to Groups A and B.

(*a*) *Aiming at natural rate.* The averages of these two groups for the natural rate of work are shown in Table I. They are exhibited graphically in Fig. 1. The figures represent the average error in millimetres.

It will be noticed that the two groups were of almost precisely equal initial ability, as judged by performance in the trial periods, but that in the subsequent tests Group B was in a marked degree superior to Group A. Its superiority is approximately 17 per cent.

This difference, moreover, was consistent throughout the eight periods and satisfies the ordinary statistical criteria of significance. On the first occasion on which different targets were employed the superiority of Group B was 17·7 per cent. The mean difference for the first half of the test periods is 16·6 per cent. and for the second half 17·5 per cent.

In view of the fact that accuracy and speed tend to vary inversely it might be expected that the superior accuracy of Group B would be secured only at the cost of some reduction in speed. This, however, is not the case. The time records of the subjects of the two groups show that the greater accuracy of Group B is maintained without any appreciable inferiority in speed. The average rate of work in Group A is 6·43 sec. per target sheet. That of Group B is 6·66 sec.

Both groups show improvement in speed as well as in accuracy. In Group A the average rate of work in the first half of the test (excluding the trial period) is 6·65 sec. per target sheet, whilst in the second half it is 6·22 sec., an improvement of 6·5 per cent. In the case of Group B the corresponding times are 6·85 sec. and 6·48 sec. The improvement is 5·4 per cent.

Table I.

Group	Trial periods		Test periods							
	1	2	1	2	3	4	5	6	7	8
A	5·00	5·05	5·09	4·51	4·41	4·53	4·50	4·11	4·13	3·97
B	4·89	4·86	4·19	3·75	3·82	3·71	3·53	3·59	3·57	3·08

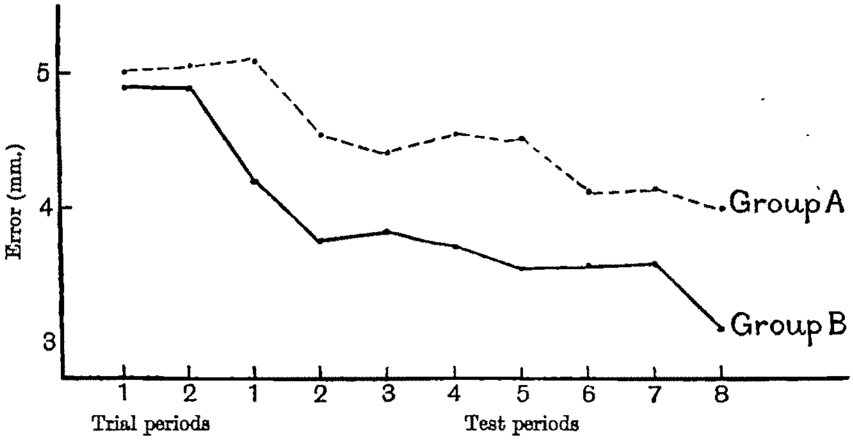

Fig. 1.

Since efficiency involves the combination of accuracy with speed, it is of interest to compare the performance of the two groups in terms of the product of time and inaccuracy. This measure exhibits more adequately the effect of practice in the test operation, but in consequence of the similarity of the two groups in respect of speed the conclusion drawn from the figures relating to accuracy alone remains unaffected.

(*b*) *Aiming at controlled rate.* That a higher level of accuracy may be obtained with constancy of speed through the use of Target *B* is further shown by the data provided by the tests performed at a controlled speed.

The performances of the subjects in these tests are similar to those at the natural rate of work. The mean superiority of Group B is 14·5 per cent., Group A showing an average error of 4·15 mm., Group B an error of 3·51 mm. In this case the superiority of the group working with the small targets is more marked in the earlier stages of the practice, being 16·5 per cent. in the first half of the week and 12·5 per cent. in the latter half.

(*c*) *The work curves.* The standard task consisted of six rounds of fifty shots a round. It is to be expected that the average performance of successive rounds will vary in consequence of such factors as 'warming up,' fatigue and incentive conditions. The influence of factors of this kind is reflected in the work curve; and it is of interest to compare the two groups in this respect.

Table II shows the average errors in each round for Groups A and B. The comparison is exhibited graphically in Fig. 2.

Table II.

	Rounds					
Group	I	II	III	IV	V	VI
A	4·49	4·37	4·32	4·21	4·15	4·16
B	3·66	3·66	3·67	3·54	3·51	3·55

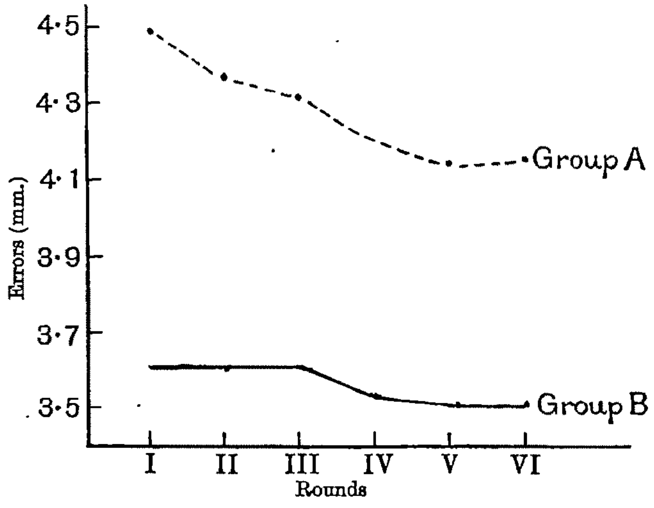

Fig. 2.

Rounds I to III were performed at a natural rate of work, rounds IV to VI at the controlled rate. There was little difference between the performance under the two conditions. This was to be expected, since the average natural rate of work was approximately the same as that of the controlled rate.

Comparing the forms of the curves, it will be noted that they are very similar. Group B is slightly more constant than Group A. With the large target improvement is more marked than with the smaller target. This may be due to the fact that in this case, performance being poorer at the outset, there was greater scope for improvement. Alternatively, it might be said the smaller target evokes greater effort at the outset, so that the level of maximum efficiency is more quickly attained. This interpretation is supported by the fact that in the first round Group B improves more rapidly than Group A through the five successive sets.

There are several indications[1] that the superiority of performances with the small targets may be in part due to factors such as (*a*) acceleration in the rate of 'warming up,' (*b*) the prolongation of effort (or increase in resistance to fatigue), (*c*) intensification of initial and terminal spurts.

(2) *Results of the dart-board tests.*

(*a*) *The principal experiment.* (i) *Practice curves of two groups.* The original purpose of the trial and re-test periods at the intermediate position of 2·75 yd. was (*inter alia*) to ascertain whether any superior degree of improvement due to practice from the more distant stance would be transferred to performance at the nearer stance. Such transference was in fact indicated, but the series was too short to provide conclusive demonstration of this.

The chief interest in the results lies in a comparison of the relative accuracy of the two groups, measured in terms of the angular deflection of the dart from the line of projection, *i.e.* from the subject to the centre of the bull's eye. The differences are shown in Table III and Fig. 3. During the first three test periods no significant differences are discernible between the two groups. Thereafter, however, Group B shows marked superiority, approximately 14·5 per cent.

(ii) *The work curves.* The experimental routine consisted of ten rounds of ten shots a round. Table IV and Fig. 4 show the average errors in each round for Groups A and B. An interesting feature in this table is that with the nearer group efficiency tends to deteriorate throughout the test period, whereas with the distant group the general tendency is one of improvement. It is not unreasonable to suppose that this difference is one of the principal reasons for the over-all superiority of the distant group. The more difficult task induces more *sustained effort*; the subject remains for a longer period keyed up to a higher pitch of endeavour and yields less readily to ennui or fatigue.

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[1] Supported by the results of subsidiary experiments not included in this report.

Table III.

Group	Test periods							
	1	2	3	4	5	6	7	8
A	2·55	2·53	2·45	2·50	2·38	2·58	2·40	2·53
B	2·55	2·43	2·57	2·12	1·98	2·08	2·13	2·27

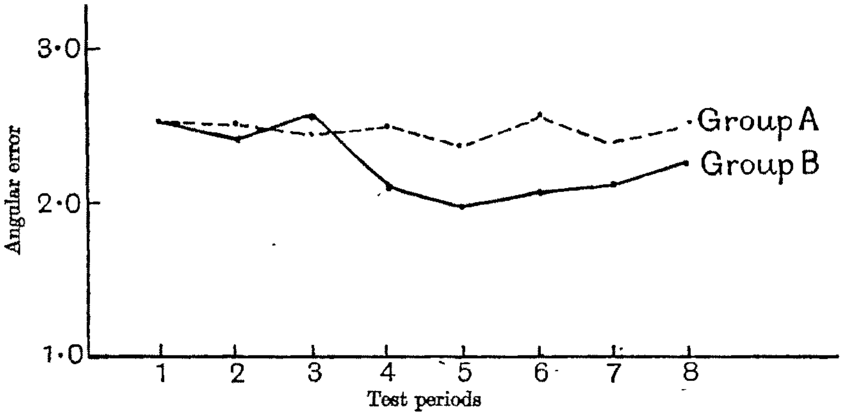

Fig. 3.

Table IV.

Group	Rounds									
	1	2	3	4	5	6	7	8	9	10
A	2·37	2·27	2·57	2·58	2·42	2·50	2·37	2·85	2·78	2·97
B	2·97	2·65	2·45	2·47	2·30	2·62	2·37	2·28	2·75	2·73

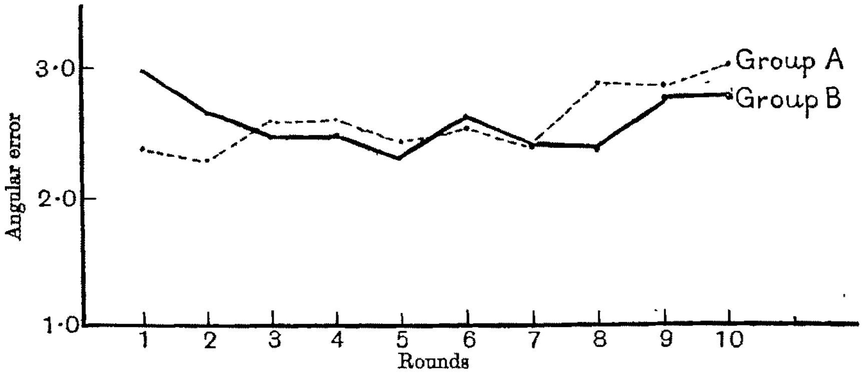

Fig. 4.

(*b*) *The subsidiary experiment*

The results for the experiment in which two distant targets were employed are shown in Table V and Fig. 5. The figures represent average errors in inches. Here the superiority of the smaller target is most clearly exhibited. The mean superiority of Group A is 13·2 per cent. The consistency of this superiority is shown by the fact that there are only five of the forty periods in which performance with the larger target is more accurate.

Table V.

	Test Periods							
Target	1	2	3	4	5	6	7	8
A	3·84	3·45	3·86	3·49	3·89	3·50	3·60	3·72
B	3·21	3·30	3·33	3·07	3·37	3·04	3·02	3·12

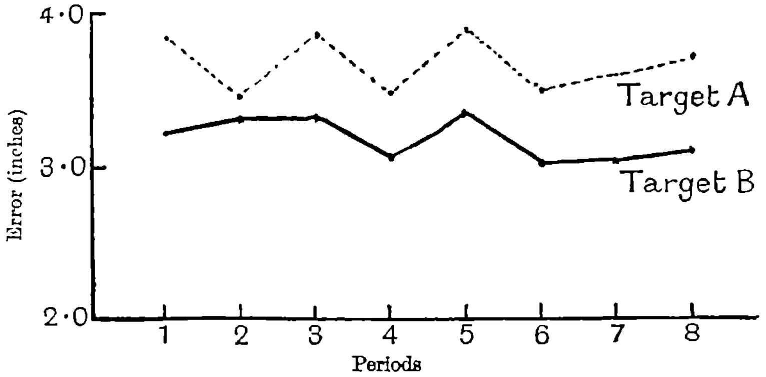

Fig. 5.

IV. INTERPRETATIONS.

(1) *The relevance of implicit standards.*

The bare fact which seems to be adequately established by the experiments is that efficiency in two superficially similar types of aiming operation varies with the size of the targets. That this is in any way due to the influence of objective conditions upon subjective standards is an interpretation. It is not incompatible with other interpretations. Possibly there is some superiority in respect of visibility and distinctness in the smaller target. Purely sensory factors are of course relevant to efficiency in aiming generally, but at present there is no evidence that purely sensory factors are alone sufficient to explain the superiority of the smaller target. Gestalt psychologists might be inclined to emphasize the importance of the configurational properties of the different targets employed. There is, for example, the important difference between the objective and its 'ground.' According as the bull's-eye is surrounded by nine or four equidistant rings, the same point will fall within the target or be referred to the background. But the difference is significant only in so far as it provides a perceptual content in terms of which an *intention* is defined. If the intention is to strike 'at least within the target'—and introspective evidence suggests that this intention is almost universally present—the perceptual difference is also a difference in the object of conation.

The slight superiority of Target *A* over the Trial Target may, perhaps, be similarly explained. It is not unlikely that the inner ring 5 mm. from the bull's-eye provides a clearer line of demarcation between the 'goodish' and the 'fair' shots than is provided in the Trial Target, or that at a certain stage of practice the inner ring provides the limits for shots recognized as satisfactory.

(2) Modus operandi *of indirect incentives.*

To explicate in detail the means whereby the indirect incentive operates would require a full discussion of the psychophysics of co-ordinated movement and of the way in which smooth muscle and glandular responses may co-operate in determining the point of transition from variability of response to stabilization. Here physiology is only a little less speculative than metaphysics.

Empirically, the chief relevant facts would appear to be the following: (1) the marksman *wants* to produce a certain effect; (2) when he achieves it he experiences satisfaction, when he fails he is discontented; (3) successful responses are more likely to be repeated, unsuccessful responses less likely; (4) the circumstances under which success and failure are experienced are conditioned by the size of the target employed. The implication would seem to be that correlated with the experience of success some physiological mechanism is operative in stabilizing the response, and that in failure there is a connected mechanism which favours continued variation. But clearly there is much more that we should like to know.

V. OUTSTANDING QUESTIONS.

(1) *Range of application of the principle.*

The *ability* to produce a given sort of effect is commonly specific. There is no reason to suppose that the *will* to produce this effect is specific in the same way, or that factors which modify the will are specific to any given objective. An incentive condition which operates favourably in one kind of manual dexterity may be expected to operate to a comparable extent in a great variety of other forms of skill. There is no reason to expect a high coefficient of correlation between the tests employed in the foregoing experiments and, say, skill in musketry. It would, however, be surprising if the results established with regard to the influence of indirect incentives upon the former were wholly inapplicable to the latter. It is also to be expected that the principle would be applicable to operations of a widely different character. Hence arises the question of the total range of application.

It is plausible to suppose that the range of utility of a given type of incentive is restricted in the first place to a group of operations to which the worker devotes himself with the same degree of application (below the limits set by his capacity). Since the effectiveness of incentives in general depends upon the affective and conative constitution of the individual, it is to be expected that the sphere of influence of any given incentive will be wide. This, however, requires empirical verification.

The range of application of the specific type of incentive here considered is also limited by the extent to which subjective standards admit of manipulation by devices of the kind here employed. The conception of an implicit standard probably applies to every form of mental and physical work. But the task itself does not always admit of the required variation. The size and shape of the implements of production must generally be determined on other grounds. Perhaps the most generally useful of the means of indirect control of implicit standards is the systematic use of 'industrial norms'—the publication (in a suitable manner) of standards of production[1].

(2) *The optimum objective.*

The fact that a decrease in the size of the target is accompanied by an increase in accuracy suggests the operation of a law of functional dependence, the formula for which remains to be defined. The question arises whether there is a certain optimum point beyond which the principle fails to apply, or is reversed.

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[1] Observations on this point are in progress, and it is hoped will form the subject of a later report.

This question arises in many parallel cases. A case of special interest is that of the control of the subjective standards of a class of students by the objective standard of an examining board. It is a platitude that impairment of the objective standard will entail a corresponding lowering of the quality of student work submitted. With indefinite lowering of the standard of the examination there will be, up to a certain point, a corresponding decline in the student's implicit standard of work. Beyond this point there will be a decrease in the number of those to whom the merit of passing the test will seem to be worth possession. Conversely with an indefinite rise in the standard of an examination there will be an increase both in the number who seek this mark of distinction and in the quality of their work. Beyond this point there will be a decrease in the number of those who regard the prize as falling within their reach. There is thus an optimum point or range of difficulty appropriate to a given examination system.

The same principle is exhibited in the progressive scale of difficulty exhibited by games. Those that are too easy pall; those that are too difficult are abandoned in despair. There would appear to be an optimum degree of difficulty conducive to maximum satisfaction. Upon the successful determination of this optimum the popularity of a game would seem in part to depend. It would appear that this optimum is itself a moving point on the scale of performance from the early to the late stages of practice, and might perhaps be best defined in terms of the percentage improvement to be aimed at. To this statement some qualification is required. Given that a fixed ratio of satisfaction to disappointment is the desideratum, it would follow from the form of the normal practice curve that any standard defined in terms of percentage improvement must be progressively decreased. Verification of the implications of this principle should not be devoid of industrial applications. If the principle of the indirect incentive is relevant at all to industrial problems it is clear that its field of utility has been insufficiently explored.

Between the level of actual performance and the level of maximum capacity there is an appreciable margin. This is the sphere of operation of incentives. In many cases the simple expedient of the reward or penalty is ineffective or positively harmful. This is the field of the *indirect* incentive.

(*Manuscript received* 30 *June*, 1931.)

NOTE ON A CLASS DEMONSTRATION OF DARK ADAPTATION.

BY G. C. GRINDLEY.

(*From the Cambridge Psychological Laboratory.*)

THE following is a description of an easily made apparatus for demonstration of curves of dark adaptation to a class in a dark room.

The apparatus consists of a box measuring about 4 ft. × 4 ft. × 8 in. Fig. 1 A shows a view of the front, and Fig. 1 B is a horizontal section through the middle. The front *ABCD* is of three-ply wood, and in it are cut eight square windows (1—8 in Fig. 1 A), each about 3 in. square. These are arranged in a circle, as shown, each window being about 18 in. from a small central window *W*, which serves as a fixation mark. Each of the windows is covered by a thin sheet of white paper, pasted to the front of the three-ply wood.

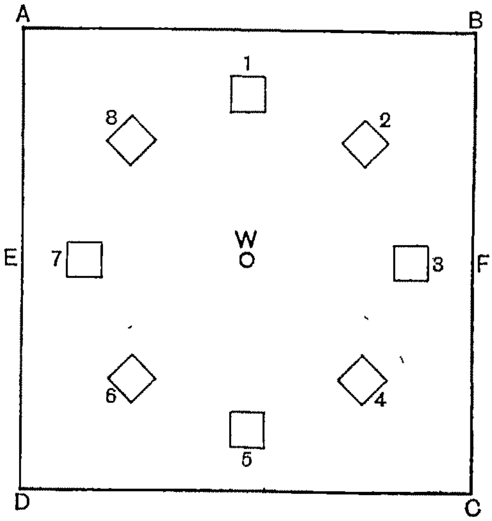

Fig. 1 A.

The sides of the box, *EH*, *FG*, are of thicker wood, and the back, *HG*, may be of three-ply wood, or may consist of a black curtain. An electric lamp, *L*, with a white glass bulb, illuminates the window *W* directly and the other windows indirectly by means of white cardboard reflectors, *rr*. Each reflector is in a small cardboard box (cigarette cartons of the kind which hold ten packets of twenty cigarettes serve admirably if postcards are used as reflectors), and the light enters through a stopping screen *SS* at the end. The stopping screens consist of pieces of cardboard having holes of different sizes, covered with thin sheets of white paper to diffuse the light onto the reflectors. It is convenient to have window No. 1 brightest, No. 2 next, and so on. The lamp *L* is dimmed down by means of a resistance *R* until the brightnesses of the windows are such that they become visible after suitable times of dark adaptation. The brightness of each window may easily be calculated from a direct measurement of its brightness when the lamp is full on, and another series of measurements of the variation of the brightness of the lamp with variation of the resistance *R*.

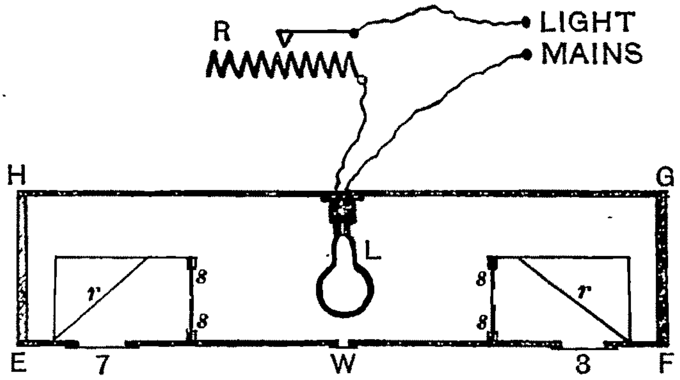

Fig. 1 B.

METHOD OF EXPERIMENT.

The class sits in a room which can be darkened, and fixates the central window *W*. The room light is then switched off, and the demonstrator calls out the time (which he reads from a dimly illuminated watch) every five seconds. The members of the class, still fixating the window *W*, see the other windows gradually appearing in clockwise order, and they note down the times at which the various windows appear. Curves may then be plotted showing the course of dark adaptation for each individual, or the average curve for the class may be obtained by finding the average time at which each window appears.

In the apparatus in use in the Cambridge laboratory the brightnesses of the windows are calculated to be equivalent to those of white screens having illuminations of about 50, 14, 7, 4, 2·5, 1·5, 1, and 0·5 × 10^{-5} foot-candles respectively. It is found that after exposure to the light of the ordinary lamps in the room in which the apparatus is set up, a fair proportion of subjects can see all the windows in less than two minutes. Fig. 2 shows an average curve for five subjects, out of a class of twenty, who were able to do this.

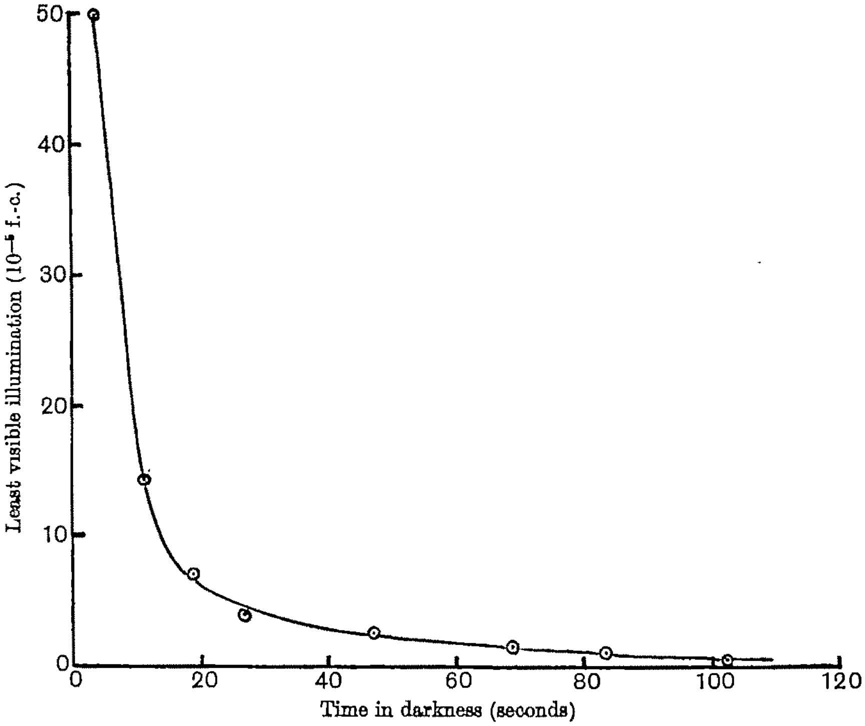

Fig. 2.

If, after a period of dark adaptation, the room light is switched on for a short time (*e.g.* five seconds) and then again switched off, the much more rapid course of the second dark adaptation can be very strikingly shown. The apparatus also serves to demonstrate the lower threshold of the dark-adapted eye for objects seen peripherally than for objects seen centrally.

(*Manuscript received* 10 *March*, 1931.)

SOCIAL FACTORS IN MENTAL RETARDATION.

BY G. M. ROBSON.

(*From the Psychological Laboratory, University of Cambridge, and Huguenot University College, South Africa.*)

I. *Introduction* (pp. 118–119).
II. *The subjects of the experiment* (pp. 119–120).
III. *The experiment* (p. 120).
IV. *General results of the association experiment* (pp. 120–123).
V. *Observations of behaviour in the association test* (pp. 123–124).
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IX. *Summary* (p. 135).

I. INTRODUCTION.

MUCH investigation has been carried on in recent years in connection with the study of defective and backward children. Most of the work has had as its aim the discovery of methods of testing for mental defect and of diagnosing the degree of defect that may exist. Besides the work on methods of diagnosis, however, a certain amount of information has been obtained about the differences that distinguish the feeble-minded from the normal individual. It is commonly stated that the defective is lacking in memory, judgment, in ability to reason, to plan, in adaptation to the companionship of others, in qualities of character, and so on. It is usual too to distinguish two kinds of defect, that termed intellectual or mental defect proper, and the kind that is called moral; the latter is often said to be connected with difficulties of social adjustment.

Some experiments on a group of children, which included a larger proportion than usual of backward children and children on the borderline of mental deficiency, suggested that the difficulties of the higher grade defective might be described as due to a lack of facility in using symbolic formulations, and that this facility is acquired at a stage of development at which the essential factor that makes for progress is the ability to communicate with others, to understand their behaviour and to enter into their experience. This sharing of the experience of other people does not come by way of an effort of thought as we know it, but by a sensitivity to social contact. This is a factor that is essential to the development of the ability to use language and to the growth of directed thought, and here the defective seems to fail.

II. THE SUBJECTS OF THE EXPERIMENT.

In accordance with the idea that backward children find difficulty in using words in a social situation, it was decided to observe the behaviour of these children in an association experiment.

Three groups of children were observed. The first group consisted of school children, of seven to twelve years old, in an industrial district in England. The district is a poor one and the children are as a whole recognized as backward by the Education Authority. They may be regarded as representative of the group described in the *Report of the Mental Deficiency Committee* as 'retarded.' The children tested were picked out by the teachers as the most backward in the school. None of them had been certified as mentally defective, but they formed a group on the border-line of mental deficiency, some of them probably falling below that line and to be classed in terms of the *Report* mentioned above as "educable mentally defective children[1]." They had all been at school for some time, but none could read or write easily, and they could not do simple sums.

It was thought that a study of differences in a group of this kind might be more instructive than a comparison of separate groups of normal and defective children. For these were children that it was difficult to classify. I was told that they were sent periodically to be examined by the Medical Officer, as in the opinion of the teachers they were incapable of profiting by the ordinary school education, but they had not so far been certified as defective.

The second group of subjects were English-speaking children in South Africa, who were living in a very similar environment to that of the first group. Eighteen children were examined, their ages varying from seven to thirteen years. Here too the children were chosen by the teachers as the most backward in the school.

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[1] "A large proportion of certifiable mentally defective children remain uncertified... and continue to attend the ordinary Public Elementary Schools." "The numbers of these certifiable mentally defective children...amount to...nearly six times as many as those attending certified special schools." *Report of the Mental Deficiency Committee*, 1929, p. 89.

The third group consisted of twenty certified mental defectives in an institution for the feeble-minded. Their ages varied from eight to thirteen, with mental ages from four years to nine and a quarter. These were selected as the children in the institution who were nearest the normal.

III. THE EXPERIMENT.

The subjects were given some practical tasks, and then an association test, in which they were told to listen to the word spoken by the experimenter and to say the first word they thought of in reply.

The responses were classified as follows:

(1) Failure to make any response at all, that is, keeping silent for thirty seconds.

(2) Repetition of stimulus word.

(3) Non-logical responses, those in which there is no apparent connection between stimulus and response.

(4) Sound responses, where the subject gives in reply a word rhyming with the stimulus word, or one of the sounds of the stimulus word, or a word beginning with the same sound as the stimulus word.

(5) Responses to preceding stimulus or response words.

(6) Multi-verbal responses.

(7) Responses in which some connection of meaning can be traced between stimulus and response.

Responses (1) to (6) may be regarded as failures to respond to the meaning of the stimulus words.

All the above types of response as well as others have been described by Kent and Rosanoff[1].

IV. GENERAL RESULTS OF THE ASSOCIATION EXPERIMENT.

It was found that in the experiment with the English children, the responses of eight were predominantly of types (1) to (6). Eight, that is to say, failed to respond to the meaning of the word, but gave responses of other types (Table I A). The results of the second group showed that thirteen out of the eighteen South African children failed to respond to meaning (Table I B). So that out of the first two groups of uncertified children, thirty-four in all, twenty-one failed to respond (62 per cent.), while out of the group of twenty certified children the failures amounted to twelve (60 per cent.) (Table I c).

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[1] Kent and Rosanoff, "A Study of Association in Insanity," *Amer. J. of Insanity*, p. 67, 1910.

Of the mental defectives the intelligence quotients of the failures varied from fifty-two to seventy-two, while those who succeeded had intelligence quotients of from sixty-seven to eighty-five, with one of fifty-five. The mental ages of the failures ranged from 4·9 to 7·8, with one of 8, while the ages of those who succeeded ranged from 7·2 to 9·3, with one of 6·2. In both intelligence quotient and mental age the two groups overlapped to some extent, but on the whole the classification according

Table I A. *Associations of sixteen English children (all girls).*
(100 *associations classified.*)

No. of S.	Age	Types 1–6	Type 7
1	8·1	36	64
*2	9·6	72	28
*3	7·9	76	24
*4	8·8	72	28
*5	10·8	92	8
*6	9·11	100	—
*7	10·6	96	4
*8	8·0	100	—
9	9·3	32	68
10	10·0	40	60
*11	10·2	72	28
12	7·5	36	64
13	8·0	40	60
14	11·0	44	56
15	10·0	24	76
16	10·5	40	60

* Those who failed to respond to the meaning of the stimulus words.

Table I B. *Associations of eighteen South African children.*
(100 *associations classified.*)

No. of S.	Age	Sex	Types 1–6	Type 7
*1	8·5	M	92	8
*2	12·4	M	100	—
3	9·0	M	4	76
*4	8·4	M	100	—
*5	8·7	F	68	32
*6	7·0	M	100	—
*7	8·0	M	100	—
*8	7·0	F	100	—
*9	10 0	F	52	48
*10	12·6	M	72	28
*11	9·0	F	96	4
12	10·8	M	44	56
*13	11·9	M	96	4
*14	11·11	F	88	12
*15	12·11	F	100	—
16	12·3	F	12	88
17	11·4	F	12	88
18	9·11	M	12	88

* Those who failed to associate to the meaning of the stimulus words.

to both intelligence quotient and mental age corresponded with the grouping according to success or failure in the association test (Table II *a* and *b*).

I did not have an opportunity of classifying the elementary school children according to mental age, but so far as could be judged from their school work and their performance in the practical tests, there appeared to be very little difference between the performance of the most backward children in the elementary school and that of the children who had been classified as higher grade defectives. It seemed then that these subjects might all be grouped together as children on the borderline of normality.

Table I c. *Associations of twenty mental defectives.*

No. of S.	Age	Sex	Types 1–6	Type 7
*1	10	F	100	—
*2	10	M	60	40
3	10	M	20	80
4	10	M	24	76
*5	10	M	100	—
*6	8	M	96	4
*7	8	M	100	—
*8	8	F	100	—
*9	9	F	100	—
10	9	F	52	48
11	11	M	8	92
12	11	M	32	68
*13	11	F	96	4
*14	12	M	100	—
*15	12	M	68	32
16	12	M	—	100
*17	11	F	100	—
18	11	F	36	64
*19	12	M	96	4
20	12	M	40	60

* Those who failed to respond to the meaning of the stimulus words.

The classification of responses used here is very similar to that described by Otis[1], and Table III shows the results classified according to the form Otis used. Type 5, however, includes here not only 'normal' responses but all responses that could be regarded as responses to the meaning of the stimulus words.

The children all appeared to enjoy the test and seemed anxious to obey the instructions they were given. I think there is no doubt that their attention was upon the experimenter, and, except in the case of two children, who did not appear to understand the instructions at all, they seemed to be attending to the actual stimulus words. But, in spite of this, in thirty-three cases out of fifty-four the responses were seldom determined by the meaning of the stimulus words. Only 12 per cent. of the associations of these thirty-three children were determined by meaning. The typical response of the backward child, the type of response given in 35 per cent. of the associations of the whole group of children, is one that bears no logical relation to the meaning of the word. Now the normal response is a word that has some logical connection with the stimulus word, and this type of response, it is generally agreed, is fairly well established in normal people by the age of seven. It appears then that there is a stage of development at which the normal type of response in the association experiment is not yet established.

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[1] "A Study of Association in Defectives," *J. Educ. Psychol.* VI, 1915.

Table II. *Dominant type of response of twenty mental defectives, classified according to* (a) *mental age,* (b) *intelligence quotient.*

(*a*)

M.A.	Types 1–6	Type 7
4	3	—
5	4	—
6	4	1
7	1	2
8	1	2
9	—	2

(*b*)

I.Q.	Types 1–6	Type 7
50	4	—
55	2	1
60	2	—
65	4	1
70	1	3
75	—	1
80	—	1

Table III. *Children classified according to associative type* (*Otis classification*); (a) *thirty-four backward children,* (b) *twenty mental defectives.*

(*a*)

Age	Failure	Type 1	Type 2	Type 3	Type 4	Type 5
7	1	1	—	—	1	1
8	—	1	3	—	2	2
9	—	—	2	1	—	3
10	—	2	2	—	—	4
11	—	1	1	—	—	2
12	—	3	—	—	—	1

(*b*)

Age	Failure	Type 1	Type 2	Type 3	Type 4	Type 5
8	—	1	—	—	2	—
9	1	—	—	—	—	1
10	—	—	1	1	1	2
11	—	—	—	—	2	3
12	—	—	—	—	3	2

V. OBSERVATIONS OF BEHAVIOUR IN THE ASSOCIATION TEST.

The behaviour of the children who were subjects of the association experiment presented some interesting features. For example, a child might, with every appearance of effort, and often of excitement, repeat the stimulus word. The word is quite obviously not treated merely as a sound, for the same child may repeat words or figures when asked to do so, quite easily and readily. Here, at any rate at the beginning of the association series, an effort is made, the response is slow, and often of this form:

Stimulus.	Child.	*Response.*	Yes, child. (8 sec.)
„	Window.	„	I also say, window. (6 sec.)
„	Window.	„	Yes, window. (7 sec.)

It seems then that though the word is not treated just as a sound to be repeated, it does not arouse associations of the usual adult type. Possibly some concrete image is aroused, and this, being unanalysed by the child, may occupy his entire field of attention. Some of the responses made by children seem to suggest that associations sometimes come by way of arousing concrete images, some characteristic or part of which is named in the response. Perhaps it may be that there is a stage of development at which words have a strongly emotional tinge, being closely bound up with the child's wants, with concrete situations. So that if the word produces a change in behaviour at all, the change is comparable to that produced in us by a word which touches off some highly emotional experience, resulting under the conditions of the experiment in a long reaction time, and sometimes in a repetition of the stimulus word.

Another type of response that is noticeable is the response that presents a logical association to a former stimulus or response. Here we have associations of the usual adult type, but they come at the wrong time. The stimulus word is treated as a signal for response and a word is uttered in reply, one connected not with the stimulus word but with the child's train of thought. But it appears that a stimulus or a response may start a new train of thought and words connected with this may be given later in response to quite other stimulus words. It seemed that the rate at which the stimulus words were given had something to do with the frequency of this type of response. The more slowly the stimuli were given the less chance there seemed to be of this kind of run-on association.

A careful experimental study of all these types of response might throw a good deal of light upon the development of the thought processes.

VI. ASSOCIATION RESPONSES OF NORMAL AND FEEBLE-MINDED.

Differences between the responses of normal and feeble-minded in the association experiment have been recorded by various experimenters, notably Eastman and Rosanoff[1], Otis[2], and Goett[3].

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[1] "Association in feeble-minded and delinquent children," *Amer. J. of Insanity*, p. 69, 1912.

[2] "A study of Association in defectives," *J. of Educ. Psychol.* VI, 1915.

[3] "Associationsversuche an Kindern," *Zeitschrift für Kinderheilkunde*, 1911.

There appears to be agreement that the responses of the feeble-minded are characterized by a relatively large proportion of reactions of the type to which I refer here, repetitions of the stimulus word, non-logical responses (no apparent connection between stimulus and response), responses by sound, as well as failures to respond at all (remaining silent or saying, "I don't know"). But it is suggested that as responses of normal individuals show all these characteristics the association experiment is not of much value in the investigation of mental defect. Now if the experimenter seeks to draw conclusions from a mere inspection of the responses given by normal and feeble-minded the criticism is probably justified. Undoubtedly normal children and adults do at times fail to respond to the meaning of a word in the association experiment, that is, they treat the word merely as a sound or a signal for any preferred reaction. But observation of the behaviour of the two groups of individuals makes it clear that responses that appear alike at first glance are not always to be grouped together. A repetition of the stimulus word, for example, may be due to boredom, dislike of the experiment for temperamental reasons, preoccupation with some train of thought, and many other causes, besides an inability to grasp the meaning of the word. Only the experimenter can decide if a subject is ready to co-operate, has an antagonistic attitude, or is preoccupied at the time of the experiment. And I think a skilled observer will in the main readily distinguish the difference in attitude of the child intent upon some problem of his own and the child who is unable to follow out a train of thought at all. A comparison of responses given by individuals of very different mental levels can be of little value unless subjective conditions are taken into account. And when the habitual response of an individual shows failure to respond to the meaning of a word and one cannot account for this in any of the usual ways, the fact may be taken as significant.

Another difference that has been noted in the responses of normal and feeble-minded is the relatively large number of what are called individual' as distinguished from 'normal,' responses. I found no correlation of these responses with educational ability in the case of the children I was dealing with, and I do not propose to consider the significance of this type of response. This paper is concerned only with the observation that certain children in the backward group failed to respond to the meaning of the stimulus word.

VII. SIGNIFICANCE OF THE EXPERIMENTAL RESULTS.

(a) *Mental defect and social factors.*

To anyone who did not know from the inside what language meant the responses of these children might seem on the whole satisfactory. The reaction came as a rule without hesitation. It was always to a word spoken, not to any other sound made, and the reply would seem to be appropriate, usually another word. But when we realize that the response was very seldom the usual reaction to the meaning of the word, except indirectly and after a lapse of time, we see where the children failed. Their difficulty appears first of all as a slowness in using words, in apprehending their meaning and using them appropriately, that is, as symbols. It is noteworthy, I think, that an older child, who was given a similar test, had a very fair vocabulary. Her trouble was not an actual inability to remember words or even to write them down, but a lack of ease and facility in using them in a social situation.

In Head's work on Aphasia[1] he speaks of three characteristics of integrated response:

(1) selection;
(2) evidence of the past functioning in the present;
(3) adaptation, that is, the response is directed in time and place.

In the case of the responses we are considering, if the reaction is regarded as one merely to a word spoken, that is, to a sound of a particular kind, the three characteristics of integration are present. But if we regard it as a response of a higher level, as a reaction to a symbolic formulation, all three are lacking. There is no evidence of selection of the response in view of the meaning of the stimulus word. The reaction is not adaptive. It is one in which the past, that is, knowledge of the meaning of the word, does not function; or if it does function at all, it does so inefficiently, non-adaptively, producing a change in behaviour when the situation it is fitted to meet is over and a new situation is produced, one for which the appropriate response is quite different.

This does not mean, however, that speed of association time need correlate closely with degree of intelligence. It is more likely that there are wide limits within which the association time varies in individuals who are capable of using words effectively. In the experiment the stimulus words were given at a leisurely rate and the children answered in their own time. Reactions which were not reponses to the meaning of the word often came more quickly than 'meaningful' associations. Where the stimulus word interrupted a train of thought, it resulted in a response that was as a general rule slower. Perhaps the train of associations, or succession of images, proceeds as quickly in the less as in the more intelligent, but the former cannot as readily make contact with a certain kind of immediate environment.

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[1] *Aphasia and Kindred Disorders of Speech*, Cambridge, 1926.

The stimulus word in the association experiment seems to be something between the stimulus that comes from a thing in the environment (*e.g.* a stone in the path of a walker, pieces of an object that have to be put together, a stick that may be used as a lever, etc.) and the operation of a plan by which behaviour is to be regulated that exists only in symbolic form in the mind of the thinker. The lower grade defective cannot deal with the first. Those of a higher grade can deal, sometimes very successfully, with problems that can be solved with reference at each step to actual concrete things in the immediate neighbourhood. They may be what is often called intelligent in a limited sphere, one in which symbolic formulations are not often needed.

According to the view suggested here, one might describe mental defect as a relative inability to get in touch readily with certain environmental stimuli. At the stage we are considering it is not stimuli from the physical surroundings that present a difficulty, though that is the case with the low grade defective, who is clumsy in dealing with things. Our experiments show an inaccessibility to stimuli that come from the social environment. The difficulty is one of interpretation at both stages. The imbecile fails to interpret the stimuli which come from physical objects, while the border-line defective is unable to respond appropriately in certain specifically social situations. Language is one of the most useful instruments we have to aid us in the understanding of the behaviour of others. But the use of words is not only a means to effective social intercourse; it is at the same time a result of social co-operation, and it seems that the lack of facility in using words is often to be traced to a more fundamental failure of social relationships.

(*b*) *Mental defect and interpretation of symbols.*

It may be objected that the children who fail to respond adequately to the stimulus word do not understand what they are required to do, and the implication is, I suppose, that if they did understand they would be able to respond with a logical association. But it is just this lack of comprehension that we are trying to explain. It is remarkable that these children fail to respond to the meaning of a word, while most children of their age succeed in doing so. An attempt was sometimes made directly to get some of the children to respond to the meaning of a word. They were told to 'listen carefully' before they answered. This did not seem, as one might have expected, to lengthen the association time. Then after a while they were asked if they knew the meaning of one of the words, *e.g.*

Stimulus. Cold.	*Response.* Kay.
Q. What does cold mean?	*A.* When you put your coat on.

It seemed that though they knew the meaning of the word, it was not the meaning that determined the association[1] but rather the sound itself. The word was not used easily and readily as a symbol. It might be so used, but it was used clumsily as an unaccustomed tool.

The children who failed in this experiment were not noticeably inferior to others of the group in the practical tests. Table IV shows that there was a marked difference between the groups in Test 1, that is, the jigsaw puzzle. In Test 2, in which parts of objects had to be put together, and in Test 3, a wire puzzle, in which two objects had to be separated by careful manipulation, the performance of the two groups was very nearly equal. And in the jigsaw puzzle it seemed that the less intelligent children did not understand what they were asked to do, and so failed completely. When they were shown that the pieces of wood were to be fitted together they seemed to be able to do the puzzle. So that it appeared that the difficulty was really connected with the use of words.

Table IV. *Performances in the practical tests.*

		Test 1		Test 2		Test 3		Total	
		+	−	+	−	+	−	+	−
16 English children	*Group	5	3	4	4	3	5	12	12
	Rest	3	5	4	4	5	3	12	12
18 S. African children	*Group	4	9	7	6	7	6	18	21
	Rest	5	—	2	3	2	3	9	6
20 mental defectives	*Group	5	7	5	7	6	6	16	20
	Rest	5	3	5	3	4	4	14	10
Total	*Group	14	19	16	17	16	17	46	53
	Rest	13	8	11	10	11	10	35	28

+ above median performance of the group of children.
− below ,, ,, ,,

It was remarkable that in the jigsaw puzzle, on which was represented an animal in a cage, the bars of which ran right down the picture, most of the children took no notice at all of colours, bars or the objects portrayed. This seems to me to be significant, for though a picture cannot be called a symbol, it is a stage on the road to the abstraction that leads to symbolic formulation. Much of the concrete situation is omitted, the pictured scene has to be interpreted. For it is doubtful if we 'see' an object in a picture in the same sense in which we see an actual concrete thing. It is probably truer to say that we have had to learn to see in representations on paper the objects of which we have actual experience. It seems then that the less intelligent child is not accustomed to looking for a meaning in pictorial representation, or at any rate that he is slow in finding a meaning in pictorial, as in symbolic, representation[1], for in the case of the children we are considering the disability is a relative one. They are not incapable of understanding a picture, nor yet of using language, but they are slow at it. One might say that the backward child does not, in dealing with pictures and still less in dealing with symbols, *seek* to interpret what he sees. His attitude to this kind of material differs from the normal. Teachers of backward children often express their feeling that the difficulty of the defective is to be explained in terms of attitude, or at any rate in terms not purely intellectual, for they say of these children that they can learn when they want to, when they concentrate, when one gets their attention. And I would connect this with the suggestion of Head's patient, who is described as defective in symbolic formulation (*Aphasia*, Case 23), that her difficulty was one of 'interest[2].'

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[1] "True symbolic reference postulates subsequent behaviour, or the assumption of an attitude, in consequence of the intervention of a symbol." Head, *Aphasia*, I, 523.

(c) *Free and controlled association.*

Another objection that may be made is to the effect that the conditions of the association experiment are of an artificial kind, and that as a general rule we direct our behaviour with reference to plans of our own, not to a series of unconnected words spoken by an experimenter. But if we analyse the association experiment we find that it bears resemblance to situations that we often meet in life. A word is a social construction. Response to language is response to a social environment, and the use of inner language in solving our problems is a secondary phenomenon, based upon the earlier use of language in response to the word spoken by another. The ease and speed with which we react to the stimulus word may well be an indication of ability to follow someone else's train of thought, that is, efficiency in reacting to a social environment. Following someone's train of thought involves at each step the inhibition, to some extent at least, of associations that are aroused in us by each word uttered.

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[1] Bühler in the *Mental Development of the Child* says, "the representational function of language and of pictures arises at about the same time," p. 77.

[2] "One word or sentence bearing upon something of interest will jog my attention and cause me to read a passage over and over again until I get its full meaning. But this *has* to be done; and if the interest is not there to 'jog,' the effort is not made because one is unconscious that an effort is needed."

A distinction is often made here between 'free' and 'controlled' associations. It is said that in the former the associative processes are "allowed free rein," while in the latter they are "placed under certain restrictions[1]." The distinction is one of degree only, for the instructions given in the so-called free association experiment also constitute a restriction upon the associative process. Control of association is demanded by the effort to keep in touch with the experimenter, especially as the stimulus words are unconnected with one another, and the subject is called upon to break off the train of associations aroused by any one word and to attend to the meaning of the next word given. The backward child finds it difficult to learn to do this. When he is started upon a train of associations, the stimulus word is treated as a signal for response, and he responds with some word connected with the train of thought. When he does react to the meaning of the word, the response is slow and indirect, by way of arousing a train of associations which in time takes the place of the one dominant at the moment. There is no definite inhibition or control of one set of associations in favour of another, such as is usual in the responses of the normal.

When we use the word 'control' in reference to associations we are using a term that does not primarily connote something intellectual. The control we are speaking of involves the adoption of an attitude, a readiness to follow someone else's thought or behaviour, and in its genesis the behaviour side was doubtless the more prominent. Following someone's train of thought originally no doubt meant doing what he did, and in that way sharing his experience. This control of tendency to immediate action which is involved in following another, and which life in society renders necessary would be a very favourable condition for the development of directed thought.

(*d*) *Reaction to remote objects.*

The study of high grade defective and backward children leads one to think that their difficulties may be very largely due to inability to react efficiently to objects that are remote. If this is so, we can see why they are often able to do mechanical work well, and sometimes manage to solve mechanical problems, provided that the solution can be reached by working out the problem practically, and that it is not necessary to work out any intermediate steps of the reasoning by the help of symbols.

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[1] Whipple, *Manual of Mental and Physical Tests*, Part II, p. 44.

It is sometimes said that defectives cannot work to a plan, that they can do only routine work. This is not strictly accurate, for it seems that they do work to a plan under certain conditions. Such work as basket making is not, strictly speaking, merely routine activity. Border-line children also sometimes do woodwork and drawing that might be called artistic. But it is true that they cannot deal with plans for the future. Their plan must be capable of immediate execution, and the materials for carrying it out must be near at hand. It is when they have to adjust their action to what is remote that they fail, and this is because they have not learnt to use symbols easily and freely in their thinking.

Ease and facility in utilizing remote objects is acquired by social intercourse, for the use of words largely depends upon concrete experience of situations in which words play some necessary function. Such an experience must be one that is of common interest to a number of individuals, an experience that is shared. At first we may suppose the word will be followed closely by action, and will be supplemented by inflection of voice, by gesture, and by signs of emotional excitement and actual preparations for the event to which it refers. It is only at a relatively high stage of development that we use the word by itself as a symbol, a stage at which the word has come to stand for a group of situations similar to one another in some essential respect, but differing in others.

Social interaction is as necessary for the emergence of this higher stage as it is for the lower one. The power to discriminate between situations alike in the reactions they demand to begin with but differing later, and, conversely, those calling for a different response at first and leading to similar behaviour later, must have been determined practically by social co-operation. For example, many of the situations that are important to the individual are at the same time hazardous, so that, if experience is to be varied enough to admit of the development of the ability to generalize, close association with the group is essential.

(e) *Words and images.*

Words form an integral part of social experiences, and they always have a social reference. It is true that in introspection we seem to find ourselves using images at times as equivalent to word symbols. They are, however, less efficient as an instrument of thought than words and other symbols capable of more ready generalization. Perhaps for this reason we are apt to think of them as an earlier tool that has been superseded by a more useful one. But the individual mental life that we know is so largely a product of a long course of development, in which symbolic formulation plays an important part, that it must be exceedingly hazardous to draw conclusions about a pre-symbolic stage of development from data that we owe to an introspective study of the images of the psychologist. For the image, as we know it in introspection, distinguished from percept and idea, centrally aroused by a train of thought, and unaccompanied by word or gesture, is surely a late product of development, a result and not a condition of the abstraction that comes with symbolic formulation.

For purposes of thinking we may use an image of some part of an experience to stand for the whole, or even for a group of experiences of a similar kind, but unless we have some word or other symbol to convey this to some one else we get out of touch with the common world, and if we are not to lose ourselves in our subjective experience the situation that we image must be described in symbolic terms. It seems that the defective who has not reached the stage of dealing successfully with symbols does tend to lose himself in his train of associations, whether we characterize them as images or thoughts. This shows that he has difficulty in dealing with the image, as with the conventional symbol, that is, both images and symbols function maladaptively.

(*f*) *Social adaptation and intelligence.*

Defective children are often openly unsuccessful in social relations. They do not 'get on' with other children. Parents and teachers say that they are difficult to manage and especially difficult to deal with in groups, that they need 'individual' treatment. Various reasons are given for this. The children are said to be temperamentally unstable, and it is thought that this instability often accompanies mental defect. The social defect is said to be due to a lack of *understanding* of the results of their conduct. I would suggest, however, that the primary difficulty is social, and that the lack of understanding follows from this.

The defective child, though difficult to deal with in social relations, is yet more dependent on others than the average child. I am not speaking here of the low grade defective who is dependent for physical needs on the care of others, but of the child who seems to be unable to 'think' unless he is helped. He responds to social stimuli only when the social situation is concretely before him, and though he is not quick in his response to the word spoken by another, he seems to be incapable of responding at all to the word written, a situation which is as truly a social one, but is so by a less direct reference. So a child, who can place five matches beside the figure 5 or the written word FIVE when one stands beside him and encourages him to do so, may sit inactive when told to go on doing this by himself in the case of the figures 3 or 4. And from observation of the child it would appear that this inactivity is not due to what one would call, in the case of an intelligent child, a sudden loss of interest in the operation, but to an inability to perform the response, to grasp the problem, without the added stimulus of the presence and co-operation of someone else[1].

A more intelligent child may, however, be hindered in the solution of a problem by the presence of another person. To him the social factor is no longer an essential part of problems of this kind, but has become a distracting element, as all detail is apt to be once a principle has been grasped. So it seems that in the case of the backward child the presence and encouragement of another may give to the situation the stimulus that enables the child to see the problem and so to solve it, while to the intelligent problems of that type have become sufficient stimuli in themselves.

To the defective, interest feeling attaches only to physical things and direct social contacts. These are the primitive sources of all our intellectual 'interests,' but the defective is unable to detach the feeling from the particular social situation and transfer it to things of general social import. And so for him words have relatively little meaning, and the complex systems of thought embodied in language do not exist.

The difficulty of the defective is to be regarded as a general defect which prevents the growth of adequate concepts. He can still successfully deal with problems that can be solved without recourse to trains of thinking built up by the aid of symbols. He can work with actual things, and sometimes with models, though he cannot deal with maps or plans. He may be quick at grasping spatial relations, but only in connection with concrete things. He cannot copy movements of a person opposite to him, for this depends mainly on an ability to generalize a spatial relation, to grasp that 'left' and 'right' are relative and not absolute positions in space. It is doubtful if this kind of generalization can be made without some kind of symbolic formulation. At any rate it means literally putting oneself in the place of another, and so involves the social reference of which we have spoken.

Perhaps a defect in symbolic formulation may in some cases be balanced by special gifts, proficiency in some practical skill, or in artistic ability. It is possibly the case that artists of considerable ability may be lacking in the capacity to use some kinds of symbol, and in fact it is usual to say that artists can seldom formulate the principles of their art. They appear to grasp the principle by a special kind of sensitivity to the material with which they deal. One might say that though, for the average person, advance in knowledge comes by way of symbolic formulation, for some, the most favourable condition of development is along a different line. But most people who show defects in symbolic formulation do appear to present a picture of arrested development. Very often such people are particularly difficult in their social relationships, they are subject to rapid changes of mood, their work varies considerably from day to day. They are apt to quarrel suddenly and unexpectedly. Their resemblance in this respect to the high grade mental defective suggests that the latter's difficulty in social adjustment may be largely responsible for their apparent intellectual difficulties.

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[1] "Dr Head's jogging of the attention was equivalent to interest in the subject matter when alone." *Aphasia.*

VIII. MENTAL AND 'MORAL' DEFECT

Mental defect and 'moral' defect, *i.e.* delinquency, do not always go together, yet both may arise ultimately from difficulties of social adjustment. Individuals differ in the extent to which they enter into the significance of some parts of the social world rather than others. In particular, we have to distinguish between social constructions that are concerned with the physical world and those that are derived directly from our relations with our fellows. It is possible that though they usually go together, ability to respond to the one group is not always accompanied by ease in grasping the other. It may be that the 'moral' defective is one who has proved himself as sensitive as the average individual to the social tradition, in so far as it is concerned with things, but who is relatively insensible to the factors that guide the average man in his dealings with other men. There are some aspects of social experience that the 'moral' defective does not share. Others may be able to enter into this kind of experience, but be insensitive to that which is more objectively determined, and concerned with things and their relations.

It might be interesting to study differences of this sort in connection with the readiness that backward peoples show in acquiring European culture. Our material civilization is intimately bound up with social and ethical systems, and it is possible that the backward peoples of the world can advance along the lines of material progress which we have discovered only as they are able to respond to the social as well as to the material aspects of civilized culture. The view most widely accepted in practice to-day is not in accord with this, but appears to hold that a group may keep its own traditions and social system, while it takes over from another merely the means of material prosperity. The evidence of history seems to be against such a view, but a psychological study of the social factors involved in the acquisition of culture can alone provide data for deciding the question. It is true that for a few individuals among us, the conditions of entering into social experience in its material and in its ethical aspects seem to differ, but the incidence of mental and moral defect[1] and perhaps also of mental and moral excellence would suggest that on the whole they go together.

IX. SUMMARY.

In this paper reasons have been given for thinking that the difficulties of backward children and those on the border-line of mental deficiency are based upon a social defect: this relative inability to interpret other people's behaviour and consequently to share their experience. No analysis of the factor that makes for ease in social relations has been attempted. It is clearly related to what has been called suggestion or imitation in psychological writing. But understanding the meaning of another person's behaviour comes not merely by imitation, but by co-operation. Interpreting his behaviour means understanding what he is aiming at, and the capacity for this comes by way of helping in the achievement.

Rivers takes a similar view when he writes, "The effect of suggestion is to produce in all the members of the group a mental content so similar that all act with complete harmony towards some common end." He goes on to say, "There is reason to believe that the harmony so produced is more complete than is ever produced by the common possession of an idea or other form of intellectual motive[2]." But it is possible that there may be a relation between common action, determined by suggestion, and the "common possession of an idea," so that the readiness to co-operate towards a common end of which the individual is not yet fully conscious, may be an important factor in the development of logical thinking.

By way of co-operation with others the qualities of character that we call tact and sympathy are built up, the ability to see another's point of view, qualities that make for success in social relations. It may be that from the same readiness to participate in common action comes the understanding of what other people mean, that sharing of common experience, which is a condition of all intellectual development.

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[1] "The only really satisfactory criterion of mental deficiency is the social one." *Report of the Mental Deficiency Committee*, 1929, p. 13.

[2] Rivers, *Instinct and the Unconscious*, p. 94, Cambridge, 1920.

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THE GENERAL NATURE OF ASSOCIATION PROCESSES WITHIN THE CENTRAL NERVOUS SYSTEM[1].

BY JOHN T. MACCURDY.

(*From the Cambridge Psychological Laboratory.*)

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I. ASSOCIATION PROCESSES AS RELATIONSHIPS OF NEURONIC EXCITATIONS.

THE proposition which I purpose to discuss is that we must regard the association processes within the central nervous system, which set off the obvious reactions of the organism, as consisting of relationships in the excitation of neurones, that these relationships may be differentiated one from another in virtue of the number of neurones involved and in their spatial and temporal arrangement, but that there is sufficient evidence to make untenable the view that specific neurones must be excited in order to have the relationship expressed, that is to say, to have the given reaction appear. It will be noted that this formula by no means denies the existence of localization (which is unquestioned for subcortical regions and demonstrable in lesser measure in the cortices of higher mammals), but that it also leaves room for vicarious function. It is a formula sufficiently elastic, I trust, to be accepted by quite diverse schools of psychology. Those whom their opponents call 'vitalists' can see in the relationship an attribute of something living. Those whom their opponents dub 'mechanists' can see in the relationship a property they attribute to mechanism. That it will cover Lahley's doctrine of equipotentiality of the cortex within the projection areas and within the 'silent' areas of the brain is clear, but it is not so immediately obvious that it is implicit in the statements of such a behaviourist as Hunter.

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[1] Read at a Joint Meeting of The British Psychological Society and the Cambridge Psychological Society in a discussion on "Neural Mechanism in Behaviour," opened by Prof. W. S. Hunter, February 14th, 1931.

II. HUNTER'S STANDPOINT.

Whether Professor Hunter would accept my formula is a guess I should make on purely personal grounds and I do not intend to make it, but that he ought to accept this—or some similar generalization—I am prepared to argue on the basis of his published statements. I will do this by means of quotations from his recent criticism of Lashley[1]. He concludes his argument with this rather guarded confession of faith: "I have no particular veneration for the reflex arc theory, and I certainly do not think that the reflex arc functions as a simple system of isolated conductors, and yet the general reflex arc theory conforms more nearly to the known facts than does any other." In the phrase "general reflex arc theory" Hunter must mean either a functional or an anatomical interpretation to be given to the word 'arc.' If functional, it means that the energy transmitted to the central nervous system as a result of the stimulus sets off a particular kind of disturbance in the grey matter which in turn activates the efferent neurones in a specific manner. This interpretation would be quite consistent with my formula. If the arc be anatomical then we are confronted with his statement that "I certainly do not think that the reflex arc functions as a simple system of isolated conductors." If not isolated the anatomically specific arc must derive its function, in part at least, from connections with other neurones. If we are to think this out rigorously, I believe we must find that this implies the notion of a relationship that dominates the relata, which tends to appear independently of the excitation of specific neurones. The non-isolated conductors have connections with other neurones. The latter function in other reactions than the particular one mediated by the reflex arc in question. If they did not—if they were excited only when the given arc was excited—then they would be part of the arc and the arc would be isolated. So, if these accessory neurones belong to other systems as well, they are non-specific and owe their inclusion into one system or another to the type of relationship that is dominating the area at the moment. Thus the relationship becomes primary. It is interesting that Hunter in setting out the purport of his paper quotes a claim of Lashley to the effect that the facts of learning dispose of theories which give specific function to specific neurones and quotes it as a claim that he will attack. Yet he deals with it directly only in the concluding sentence which I have just been dissecting.

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[1] *J. Gen. Psychol.* 1930, III, 455–468.

III. THE SIGNIFICANCE OF MULTIPLE STIMULI IN MAZE-RUNNING.

The bulk of his argument is concerned with a criticism of Lashley's maze-learning experiments and conclusions therefrom, together with the setting-up of the counter-claim that many different stimuli are effective in solving the problem of maze-running that "no one cortical lesion should destroy the response and the ability to form and retain the habit should be decreased roughly in proportion to the amount of cerebral tissue destroyed. As more and more tissue is eliminated, more and more cortical projection areas are removed, and the total integrative function of the cortex is increasingly hampered." Let us, for the sake of argument, accept his criticism as justified and examine the properties of the mechanism he postulates.

There are probably at least five modalities of sensation which may be utilized in learning to run a maze, but to simplify the discussion let us assume that there are three—*a*, *b* and *c*—and that, before the experiment begins, the animal has reflex potentiality for the performance of α, β, and γ, reactions which correspond to the senses *a*, *b* and *c*. *a* will stimulate reactions of the α order, *b* of the β, and *c* of the γ[1]. As a result of all the influences which result in 'learning,' the animal finally reaches a state when it can, when confronted by these stimuli, in the order and combination in which the maze presents them, produce a sequence and combination of the reflex acts, corresponding each to an original stimulus that led to its response in an unintegrated form. All these reflex acts together constitute the behaviour exhibited in successful maze-running. Let us call the final stimulus complex *abc*, and the combined responses $\alpha\beta\gamma$. What will be the effect of eliminating the stimuli belonging to the group we have called *a*? The answer will depend on what meaning is given to the phrase

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[1] In the following discussion *a*, *b*, and *c* will also stand for the neurones in the projection areas of *a*, *b* and *c*; and α, β and γ will represent the constellations of neurones that discharge to the common final paths of the reflexes so named.

"total integrative function of the cortex," a phrase which might be employed by the upholders of mutually exclusive theories.

IV. CHAIN-REFLEX THEORY UNTENABLE.

The first possibility to be considered is that each of the many reactions involved in the maze-running occurs as a separable reflex, that is, that at one moment the animal is responding to a certain stimulus in a manner specific for that stimulus, is then exposed to a second stimulus which inhibits the first and elicits a second different reflex and so on. This is the chain-reflex interpretation of continuous behaviour. If this be an adequate description of what takes place, then two results will follow. First the progress of the animal will be observed to be discontinuous and not to follow a smooth path. Spinal reflexes, for instance, which are mutually inhibitive, do not succeed each other by any smooth transition but by a complete inhibition of the first when the second occurs. Smooth transitions appear only when one spinal reflex leads over to another with which it is allied, that is, when a larger reflex integration is being evoked gradually. During reflex spread the successive elements appear in relative independence of their specific stimuli, as if, to use Hunter's simile, there were some central neural engram which was unwinding itself like a gramophone record. So far as I know there has never been reported any such discontinuity in the progress of a proficient animal as the chain-reflex hypothesis would necessitate. On the contrary, as Sherrington has pointed out, there seems to be a lowering of the threshold for each successive response, which means a selection at each stage of the stimulus (out of the myriad of sense impressions to which the animal is being subjected at every moment) appropriate to the train of reflexes which makes a unit of the behaviour. A second result of there being separable reflexes involved in maze-running would be the total disappearance of some links in the chain of reactions when one group of stimuli was eliminated as the result of destruction of a cortical projection area. This loss would, also, be permanent. There would then result an immediate and complete inability to run the maze, and when it was relearned as a combination of reflexes belonging to the other senses, the second behaviour would be different from the first—as different as the methods used by a man in looking for something in the dark and in the light. No such dramatic results have been reported, so far as I know, and it is unlikely that they would escape observation.

V. POSSIBLE BRAIN MECHANISMS.

We are therefore forced to the conclusion that, although a and b and c were all used in the formation of the behaviour $\alpha\beta\gamma$, yet, when that is once established, the elimination of a does not lead to a reduction of $\alpha\beta\gamma$ to $\beta\gamma$, but that there is still a tendency for the α element in $\alpha\beta\gamma$ to appear. $\alpha\beta\gamma$ has become a unit capable of being elicited as such without the stimuli appropriate for the α element in the integration. Let us assume that, as a result of the learning process, some mechanical system of neurones has been linked together by specific conductors for the production of $\alpha\beta\gamma$. What possibilities are there as to the general nature of this mechanism? I believe there are only three, and each of these must be examined to see if it represents a feasible scheme.

First we may assume that, since $\alpha\beta\gamma$ can appear in the absence of a, b, or c, the essential energy for the discharge of nervous excitations is resident in the nexus of neurones which send their impulses into the final common paths and that a, b and c act like triggers, any one of which can liberate the centrifugal discharge. This is a conceivable type of mechanism, but, if it were the right explanation, the removal of one projection area would result in no loss of retention for the habit. Since some degree of retention defect does ensue, the total energy requirements of the mechanism in the intact animal must include significant contributions from every one of the projection areas that receives and transmits stimuli for the integral unit reflexes that make up the total behaviour.

Let us scrutinize a second possible type of mechanism, which will take account of this factor. Significant sources for the energy of the discharges into the final common paths now include the projection areas. Since, however, a perfect performance can be relearned after one projection area is removed, it follows that, in the course of relearning, new connexions are effected from the remaining projection areas, or, what comes to the same thing, these areas become more active. The remaining projection areas in some way or another become more effective sources of energy than they were in the intact animal. It is a simple matter to have a mechanism designed to increase a supply of energy when it shows any sign of diminution. Thermostatic control of fuel or electric current is the commonest example, and we can easily imagine a similar mechanism to exist in the brain. Relearning would then be nothing but increasing the flow of gas through the remaining taps when one of a group of burners was shut off. All such devices, however, depend on the temperature, pressure, electrical potential or what not being at the critical point before the source of energy is adjusted by the automatic regulator. The critical point for the brain mechanism we are examining is that at which a discharge will be made into the final common paths, that is, at the threshold value. When the threshold is reached, the regulator comes into action—how? Like all regulators, it is an inhibitor: it keeps the temperature, potential, or whatever it may be, from rising beyond a prescribed point. Until the energy reaches a certain accumulation the taps are all wide open. If one is closed by some external agency, then the regulator will allow the others to contribute more than their previous quotas before it begins to shut them down. If there be some such automatic regulating device in the neuronic mechanism, we must assume that it is in operation in the intact animal and that it tends to inhibit the inflow of energy from each projection area so soon as the threshold for the system as a whole has been reached. This corresponds with the universal observation that in every integration of reflexes each component is inhibited of its full potential expression. We can now see how this mechanism would explain the perfecting with practice of a complicated habit which has been partially destroyed by the removal of a projection area. Let us suppose that the a stimuli are cut off from $\alpha\beta\gamma$, which we can regard as having a triangular relation, each being connected with the other two as a result of *Bahnung*. α has always received its chief supply of energy from a, but it also has acquired connections with β and γ. After the operation b and c will send more than their previous quotas of energy into the β and γ corners of the triangle, because the previous inhibition is removed, which will tend to make the β and γ reactions more prominent. Some of the energy in β and γ will, however, tend to flow along the paths to α. The oftener this happens the more will the channels from β and γ to α be canalized. When the *Bahnung* is completed the habit will be performed as well as before. This mechanism will account for re-education when some *Bahnung* between α and $\beta\gamma$ has been achieved. But it will not account for a learning of the habit when from the outset the whole of one projection area has been removed. The learning of a maze by a blinded rat, for instance, is not covered by this scheme.

To understand initial learning after mutilation we must assume another feature in our hypothetical mechanism. Essential component reflexes must all of them be capable of arousal by stimuli from more than one projection area. For instance, if any given movement essential in running a maze could be stimulated only through vision, it is clear that no blinded rat could ever learn to run that maze. Since a blinded rat can achieve this feat we are forced to conclude that the reflexes involved are capable of arousal through stimuli of more than one sense order. The mechanism that provides for this must be of one out of two possible types.

The first is an arrangement whereby a stimulus proceeding from either source is adequate to evoke the response and where the connections with either of the projection areas concerned may be integrated into the total mechanism for maze-running. In this case the destruction of one of the projection areas would have no effect whatever on educability in the maze. According to Lashley's reports, this is what happens in the brightness discrimination problem after destruction of the occipital lobes. But if either one or the other source of stimulation is used by the intact animal, then the destruction of the projection area which has been linked in with the total mechanism would lead to total loss of the habit after removal of the area, although it could be then relearned as quickly as it was originally. Lashley's observations seem to indicate that this is also true of the brightness discrimination habit. With the real mazes, however, destruction of one projection area does not lead to total loss of the habit and it does affect the rate of relearning.

The data from maze problems forces us then to consider the second possible type of organization of alternate sense stimuli into the total mechanism. In this, although the reflex in isolation can be elicited by either stimulus, its integration into the chain of reactions involves the incorporation of the pathways from both the projection areas. The particular reflex in question is then most readily evoked by stimuli which proceed from both sources. If one is blocked the lack of its contribution will reduce the efficiency of the train of responses, but this can be remedied by further *Bahnung* of the connections with the other projection area. To arrange for the reciprocal connections between projection areas and the neurones which excite the final common path, we can assume a mutual inhibition and an automatic regulator within the mechanism of the individual reflex just as we did for whole groups of reflexes above.

VI. THE PROBLEM OF QUALITY SENSIBILITY.

We have thus arrived at an explanation in terms of quite conceivable mechanisms as to how constellations of specific neurones might regulate behaviour and still exhibit vicarious function. Throw in a little neurobiotaxis to account for the *Bahnung*, and the story seems to be complete! But, unfortunately, we have throughout been tacitly assuming that each projection area contributes to the totality of associated neurones merely varying amounts of excitation, discharges that can be adequately described in purely quantitative terms. This assumption is perhaps allowable when one is dealing with such a purely quantitative stimulus as mere brightness; in this case the number of neurones in the projection area excited to the point of discharge is all that matters. The conditions are of a tropistic simplicity. But many, if not most, of the stimuli, to which man and the higher mammals respond through projicience, cannot be adequately described without using qualitative terms, or, at least, terms that cannot be translated into units of energy. The difference between a seen square and circle is not a difference in brightness and the phenomena of vicarious function in connection with pattern vision—to take only one example—make our assumption illegitimate.

The difference of kind between stimuli, which we can most easily describe as one of quantity versus quality, is probably encountered in all the senses but it is most easily discussed in connection with vision because we are all so familiar with the difference between brightness and form. An optically real image of a circle falling anywhere near the middle of the retina yields a perception of a round outline. It seems improbable that there is an accurate point to point transference of excitations from the retina to the visual area in the occipital lobes so that the circle could be projected as a ring of excitations in the occipital neurones. But let us assume that the retinal neurones, which are excited, pass on the impulses to some cortical neurones with which they are specifically connected. The retinal circle then becomes a cortical excitation localized in some specific area and having a specific shape. If there be now some of the neurones in this region destroyed, the circle previously seen in that part of the visual field will be wiped out or incomplete. Small lesions in the occipital poles do not produce such effects, however, and, furthermore, when an occipital injury has been large enough to cause some defect in the visual field, that defect may pass away completely.

VII. VICARIOUS FUNCTION.

These facts force one conclusion inevitably. It is that the functions of any given small area of the occipital cortex can be taken over by other areas without distortion of the ultimate perception. We have seen how the cortex, functioning mechanically, might succeed in doing this for mere brightness, but it is impossible that it should do so in the perception of form. If any given occipital neurone has a specific function it must be related to localization in the visual field. If there is a small gap in the occipital neurones and the visual projection area still passed on impulses as if they came from the defective area, these impulses must arise in adjacent neurones which take on a compensatory activity. If these substituting cells have a specific function, their activity will transfer what is seen through them to their part of the visual field and the outline of the circle will be distorted. The upholder of the doctrine of specific function might admit this and say that the localization of 'circle' is really in the association areas further on. But this will give him only temporary refuge, because we can see that, by exactly the same reasoning as we have been using, an injury in the 'silent' areas of the brain ought to produce gaps or distortions in the visual field, if there be specific localization, yet this never occurs.

VIII. THE DOCTRINE OF SPECIFIC LOCALIZATION UNTENABLE.

We are, then, forced back to some such formula as I began with if we are to describe the general nature of central nervous processes without inaccuracy, without the inclusion of a factor which is justified by pious hope but contrary to patent facts. I should make it clear that in my speculations as to possible types of mechanical processes, which might explain the facts and include specific function, I make no claim to have exhausted the possibilities. I have gone so far to meet the mechanists as my knowledge of the principles on which machines work will carry me, and I believe that it is as far as any mechanist has gone without indulging in tautology. The facts of vicarious function make specific function an impossibility for common sense. Common sense is not science, but it does throw the burden of proof on the man who affronts it. Let the mechanist describe his mechanical process by appeal to known mechanical principles and not, at some point, say, "The machine is so arranged that it will do so and so."

IX. THE FREQUENCY OF VICARIOUS FUNCTION.

Lashley's work has brought this discussion into the lime-light, but it should be remembered that he could have made his claim, as to equipotentiality of the cortex as a whole and within regions that do show some localization, without an appeal to his own experiments. They merely paint the lily that had already grown outside his laboratory. (I refer, of course, only to the significance of his observations for the problem of cerebral localization.) Ever since central nervous system functions have been accurately studied a demonstration of localization has been closely followed by exceptions which bespoke vicarious function. To the clinical neurologist this is a truism, and results of experimental investigations point in the same direction. So long ago as 1882

Osawa[1] reported from Goltz's laboratory that three hemisections of the spinal cord of a dog could be followed by some return of function. Recently Kroll[2] describes the return of qualitative sensibility in man after operations on the sensory conduction paths in the spinal cord. These results refer to a part of the central nervous system where functions seem to be localized in quite specific structures. In the brain, however, the localization of function is notoriously like a will-of-the-wisp. It has been chiefly studied experimentally by stimulating electrically, and operating on, the Rolandic area. At first blush there seems to be a specific localization. Cut out a piece of the motor cortex and a paralysis of some part of the body results. But, if the excised portion be not too large, the paralysis may disappear without a trace of disability. If an electrode be placed on some spot in the motor cortex there will be a localized muscular excitation in some part of the body, which seems to indicate that there is a specific cortical localization for various movements. Unfortunately, however, the results are inconstant. In the first place, if the electrode be carried along a line on the cortex and then the direction of this serial stimulation be reversed, points will be found where facial muscles will contract on stimulation in one direction, but the elbow-joint will move on stimulation of the same point when approached from the opposite direction. Again, a point which today gives a consistent response in one part of the body may transmit its excitation to another region tomorrow.

There are many of such observations, the cumulative effects of which are compelling on any unprejudiced mind. But any one of them, properly attested, should be enough on logical grounds to make the doctrine of specific localization untenable. If it is to be interpreted literally, it must be abandoned. In its place must be put some such formula as I have proposed.

X. 'ANATOMICAL DESIGNS.'

Some years ago, impressed by this evidence, I put forward a scheme[3] to enable us to envisage the general nature of such localization as there is in the central nervous system. I assumed that the discharges which proceed to the final common paths are rendered specific by virtue of their arrangement into what I called 'anatomical designs.' In any region of grey matter—large or small in conformity with the distribution of localization—a number of neurones are excited so as to make up a specific design in space and time: that is, to take an example, three neurones *a*, *b*, and *c* could make one design if all in one line, another if forming a triangle, a third if excited in the order *a*, *b*, *c*, a fourth in the order *a*, *c*, *b*, and so on with an almost infinite possibility of combinations when the number of neurones was large. So long as connections with the final common path were left, mutilation of the area in question would not affect function until there were so few neurones remaining as to make the representation of the design difficult or impossible.

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[1] Inaug. Diss., Strassburg: Schultze, 1882, cited by Fulton, Liddell and Rioch, *Brain*, LIII, 312.

[2] "Schwellenuntersuchungen bei Laesionen der afferenten Leitungsbahnen," *Zeitschr. f. Neur. u. Psychiat.* CXXVIII, 751, 1930.

[3] In *Common Principles in Psychology and Physiology*, Cambridge University Press, 1928.

XI. 'PATTERNS.'

What guides the impulses to the appropriate neurones is a mystery, but, since it is a mystery that pervades all nature, I called this agency a 'pattern.' Patterns are immaterial, abstractions, if you will, but they operate to guide the distribution of energy (having no energy themselves) in the way that a law of nature is said to govern natural phenomena. The pattern method of dealing with problems is, indeed, identical with that used by mathematicians, who abstract the properties of number and dimension from one series of observations, study their laws and then apply these laws to situations which are similar to the first only in respect to these properties. In fact I should say that mathematical laws are examples of patterns, patterns which refer merely to number and dimension. The mathematician's method of treatment can be applied to properties that are incapable of expression in mathematical terms.

XII. STATISTICAL PROPERTIES.

These speculations of mine are not out of harmony, I believe, with certain recent work. Hecht's[1] studies of retinal vision indicate that all rods and cones in any given area come into play only with the brightest illumination. The curves for brightness discrimination and visual acuity indicate that the rods and cones have varying thresholds and that the functions of the retina as a whole at any moment are determined by the average threshold for that illumination. The distribution of the cells of any given threshold is like that for the distribution of men of a given height in any population. In other words, until every rod and cone in a given area has been excited, the impulses sent by the retina to the mid-brain do not necessarily arise in any specific rods and cones. If fifty men of the height of five feet six inches or under were to be chosen in Cambridge, they could come from any one college or a scattering from all the colleges. The mathematical interpretation of retinal vision indicates this kind of distribution of excitation in the retina. This type of analysis has been extended to cover intensity discrimination in muscle sense and pressure sense[1].

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[1] *The Foundations of Experimental Psychology*, Clark University Press, 1929.

Crozier[2] and his colleagues have made mathematical analysis of the operation of stimuli in various tropisms and have shewn that the resulting behaviour is quantitatively predictable in terms of the gross amounts of differential excitation of opposed groups of receptor mechanisms. Quite recently Hoagland[3], generalizing from this type of interpretation, has suggested that the discharge from any association area in the central nervous system may represent statistical properties of the neurones in that region. Such a view may get away from specific localization and, I believe, is quite compatible with my formulation of anatomical designs. The difference is in the implication of one kind or another of guidance for the distribution. The mathematical view makes the distribution a matter of probability, *i.e.* chance. I say it is not chance but an immaterial agency which I call a pattern. Chance is, of course, also immaterial. How are we to interpret chance?

XIII. PHILOSOPHICAL PREJUDICES AFFECT SCIENTIFIC THEORY.

In asking this question a factor is revealed which is fundamental to an understanding of the differences that lead to heated polemics. The question is not a scientific but a philosophical one. If one's professed and intuitive philosophy are to be the same, the answer will profoundly affect one's outlook on life. But the scientist, *qua* scientist, ought to have no interest in it. That he is interested shows that his devotion to one or another theory is motivated in part at least by his *Weltanschauung*, emotional faith is influencing intellectual hypothesis. When the mathematician tells us that the nature of a physical system changes with the location and method of examining it, some of us can accept the statement as a mathematical truth and leave it at that. Others, whose faith is crudely materialistic, deny that a material system can be influenced by something that has no material connection with it and exclude this datum from their interpretation of nature. Still others find in this a proof of immaterial influences which they may amplify until it is made to justify the existence of a personal God.

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[1] Hoagland, "The Weber-Fechner Law and the All-or-None Theory," *J. of Gen. Psychol.* III, No. 3, 351.

[2] *The Foundations of Experimental Psychology*, Clark University Press, 1929.

[3] *Loc. cit.*

XIV. THE VARYING IMPLICATIONS OF COMMON TERMS.

I believe it is useful to recognize the existence of these wholly personal factors, because it is they which prevent agreement in the interpretation of undisputed facts. Opponents may agree as to the accuracy of an observation, they may even agree as to the definition of the term used to label the phenomenon, but will begin to disagree when that term is used in any generalizations. There can be only one explanation of this and it is that the implications of the term are different for the two disputants. It may be useful, therefore, to conclude with an examination of the implications, in opposed schools, of terms in common use.

XV. MISUNDERSTANDING BETWEEN MECHANISTS AND VITALISTS.

These schools I will call the mechanists and the vitalists, but even these terms have to be defined. My definitions will be that a mechanist is one who holds that living organisms are complicated machines and exhibit no functions which cannot be accounted for, theoretically, by a mechanical system that is sufficiently complicated; and that vitalism is "The doctrine or theory that the origin and phenomena of life are due to or produced by a vital principle, as distinct from a purely chemical or physical force." The first corresponds closely to that given in *The New English Dictionary*, while the second is quoted from that authoritative source. Practically 'vitalism' has become so closely associated with one narrow immaterial agency that few of those whom I am now dubbing 'vitalists' would accept the title; but that matters little for the moment. The implications of these terms within the two schools are quite different. The mechanists seem to argue that belief in an immaterial agency is incompatible with anything but an animistic interpretation of nature. The vitalist believes in spooks or, more respectfully, is a 'mystic,' which, through some strange perversion of the term, seems to mean a spiritual interpretation of nature. Were these implications justifiable, there is no doubt that 'vitalism' would be unscientific. Believing the word to carry such implications, the mechanists are quite right in excluding vitalism. On the other hand, the vitalists believe that mechanism means a machine in the narrowest sense and that the vision of the mechanists is perverted when they cannot see the existence of non-mechanical factors in biological processes. But does 'mechanism' imply nothing further to the mechanist?

According to the *N.E.D.* mechanism refers to the properties of machines and that word is defined as follows: "An apparatus for applying mechanical power, consisting of a number of interrelated parts, each having a definite function." Such a definition excludes the idea of a relationship which can dominate the relata, a relationship which is more than the algebraic sum of the functions of the parts. It is probable that to the biologist of a generation ago, when crude materialism was a universally accepted scientific dogma, this was the meaning of mechanism, as it still is for the vitalist school. But in the minds of the mechanists the meaning has been gradually extended and by two processes. The first, and a scientifically more respectable one, is a subtle logical error. Since mechanism is best studied with the aid of mathematics, anything which can be expressed in mathematical terms is held to be mechanical in nature. Modern physics has shewn that this is palpably untrue or that the meaning of mechanism must be extended. If extended, it brings in the idea of a relationship dominating the relata, and further discussion, as we have seen, becomes philosophical and not scientific. The other process shews this reasoning: the principles of nature are mechanical, every function of an organism is therefore mechanical; matter is plainly present in the organism, therefore the function observed must be a function of that matter. If one is intuitively reasoning in this way, I can sympathize with the feeling of insult to intelligence that a vitalistic interpretation brings. To say that an influence is at work that is immaterial is tantamount to denying that the material parts of the organism have any function at all, because, if those parts are not producing the function of the organism, what good are they? Each side in the controversy thinks the other is blind to plain facts, whereas the truth is that the blindness is not for facts but for the implications in the minds of the opponents of the terms used in common.

A RESTATEMENT OF THE PROBLEM OF LEARNING.

By DONALD K. ADAMS.

(*Duke University.*)

I. *Introduction* (pp. 150–151).
II. *Certain properties of organisms* (pp. 151–158).
  (*a*) *Needs* (p. 151).
  (*b*) *Parsimony* (pp. 152–157).
  (*c*) *Retentivity* (pp. 157–158).
  (*d*) *Sensitivity* (p. 158).
III. *On the nature of the learning process* (pp. 158–165).
IV. *Varieties of adaptation* (pp. 166–176).
  (*a*) *Changes in the need* (pp. 166–167).
  (*b*) *Changes in the field: learning* (pp. 167–176).
V. *Summary* (p. 176).

I. INTRODUCTION.

THE present chaotic state of the psychology of learning affords an ample demonstration of the urgent need for a re-examination of current formulations of the problem.

The best introduction to the restatement here to be advanced is a brief critical consideration of certain general properties of the class animals, with some mention of those facts which are known as the 'laws of learning.' For the purposes of this critique the discussion will be more or less limited to the relatively unambiguous types of learning which, incidentally, are those upon which the 'laws' are for the most part based, and of which the learning of animals in puzzle boxes, mazes, *détour* problems and the like is typical. It will be a relatively simple matter later to indicate the position in this formulation of the more equivocal forms of modifiability[1], such as adaptation, conditioned reflexes, etc. It will also not be difficult to extend these considerations from apes, cats and rats to both ends of the animal kingdom. So when the reader has an impulse in the following introductory critique to insist: "But here is a type of learning to which these considerations do not apply," I can but ask him to be patient and to reserve his objections for a later section, with the assurance that this temporary limitation is adopted only in the interests of clarity and brevity of exposition.

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[1] The term 'modifiability' seems to be a lumping or confusion of the four properties discussed in the next section.

II. CERTAIN PROPERTIES OF ORGANISMS.

In my opinion, learning is a *real* process (the exact nature of which will be defined in due course) and not the superficial and merely technologically defined concept for which it is so generally mistaken. Further, learning is not only a real, but a *psychological* process, a fact that psychology seems to have forgotten. Finally, it is a *process*, and not to be confused with certain *properties* which we shall have to discuss. Failure to make this distinction is one of the important factors in the present confusion of thinking about learning.

(*a*) *Needs.*

The first property of organisms to which attention must be called is the obvious one that they need things, *i.e.* they have *needs*. This admits of no argument. Sticks and stones do not need things, but organisms do. These may be air, water, light, wealth, pearl necklaces, motor cars or what not. Some of these needs, namely those that are common to or characteristic of large groups (*e.g.* species) of organisms and that recur frequently through fairly large parts of their life cycle, have been called instincts.

This is, I believe, the essential basis of the distinction made by Lewin (27) when he speaks of 'real' needs and 'quasi-needs.' The former I take to be what are usually called instinctive needs, and the latter the less common ones. Perhaps they could better be characterized as specific (*i.e.* common to a species) and individual needs, but the distinction has, at least for our present purpose, no great importance. Certainly the difference between them is not one of reality or genuineness.

The needs of organisms have no necessary relation to consciousness. They may or may not be represented in consciousness by wishes, desires, etc. Behaviourists especially talk a great deal about their doctrine as being a branch of biology, which of course it is not. Psychology, however, *is* a biological field, and one of its fundamental premises must be the recognition that a tree's need for light, a cat's need to escape from the puzzle box and a psychologist's need to put her into it are but different manifestations of the fundamental biological fact that organisms need things.

(*b*) *Parsimony.*

The second property of the class animals which we have to discuss is not so easily described. Although a fundamental property, co-ordinate with that of needs, it has been only partially recognized, and even that by only a few investigators. Consequently we shall have to approach it in a more roundabout way.

In attempting several years ago to give a general description of the adaptive behaviour of cats in a considerable variety of situations, the following appeared to be the essential features of the process:

1. There is a cat.
2. There is something the cat wants[1]. The thing wanted may be a state of affairs, such as freedom from constraint, and the thing may be wanted very much or very little.
3. There are other things too. Nearly everybody calls these the situation. The disposition of these other things may be such as to facilitate or to impede the cat's getting what it wants.
4. In one way or another, after certain movements[2] with respect to the situation and the thing wanted, the cat gets what it wants.
5. There is the cat again.
6. There is something the cat wants.
7. There is another situation, *more or less* like the first. This similarity may be very great [the 'same' situation] or very slight.
8. The cat gets what it wants more readily than it did before, that is, with greater economy of movement[3]. This economy may be small or great ((1), pp. 155–157).

Now this economy is, under certain conditions (*i.e.* repetition), a characteristic *behaviour sign* of learning in just the same way that spherical shape is, under certain conditions, a sign of surface tension in a drop of fluid. Moreover it is *merely* a sign of learning. It is not necessary for the act ever to be repeated in order that the learning process should take place, any more than it is necessary for a drop of liquid to be in the situation in which it assumes a spherical shape in order that the process of exposing the least possible surface may occur. The spherical shape of raindrops or of molten lead, in falling, may very well have been the sign that led to the discovery of the fact of surface tension; and the economy of movement upon repetition of an act may serve in the same way to draw our attention to the fact of learning. But to consider that this economy *is* learning, as is usually done, introduces at the very outset a fundamental confusion between the process and one of its contingent or conditional behaviour signs.

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[1] Prof. McDougall suggested shortly after the appearance of the above formulation that the word 'need' would be more accurate and comprehensive and less ambiguous than 'want.' He was right, and the word 'need' is here used throughout. It is necessary also to mention Wallace Craig's very acute studies of the dynamics of behaviour. These studies, especially that on appetites and aversions (6, 7, esp. 8), seem never to have commanded the hearing they deserve, perhaps because our ears were filled with the sound and fury of behaviourism. Lewin's (28) ingenuity has more recently opened up this whole field (in humans) to experimental investigation.

[2] In man actual movement may not be involved, and the whole field, including the needed object, may be purely ideational or symbolic. We make no real error and have the advantage of concreteness if discussion is restricted for the moment to non-human animals and actual movement. As will be abundantly evident, the same considerations hold for both cases.

[3] Unless anticipated, the objection will probably be raised that this economy upon repetition does not always occur, that the second trial may take longer than the first, that the tenth may display more movement than the ninth. These are 'accidental' in the truest sense of the word: that is, they are due to undetected differences in the animal or the situation. Either the situation or the need is not the same as in the preceding trial.

Economy upon repetition is the most convenient and accessible and perhaps also the least unreliable sign of learning. Under certain conditions it may even be, and has been, used as a 'measure' of learning, but it must be used with extreme caution. Measuring this economy in terms of elapsed time required to execute an act is notoriously unsatisfactory; measuring it in terms of 'errors' is somewhat less so. In any case what is measured is not learning but economy, which is no more than a contingent behaviour sign of learning. This is often, to be sure, the best we can do, but it must be remembered that learning is not being measured directly and that it occurs in the absence of the condition usually used to demonstrate its sign, namely, of repetition.

Further, this economy upon repetition or, better, the property (we shall call it *parsimony*) of which it is simply one manifestation, is a fundamental property of a certain class of bodies (namely, animals and probably all organisms[1]) in just the same way that surface tension is a property of another class of bodies (liquids). *We need not and cannot profitably inquire further.*

Thorndike included among his classical experiments on cats some in which the door of the puzzle box was opened by the experimenter when the cat licked or, in some cases, scratched herself. He found, in spite of the crude measure (time) of economy used, marked differences between the learning curves of these cats and those of the cats that learned to escape from puzzle boxes the doors of which were opened by some effort of the cat herself, such as pulling a loop, pressing a pedal or turning a button. We are not here primarily concerned with this difference, to which we shall have later to return, but with another even more significant. He observed that "there is in all these cases a noticeable tendency, of the cause of which I am ignorant, to diminish the act until it becomes a mere vestige of a lick or a scratch..." ((38), p. 48).

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[1] I think that parsimony is a property of all organisms, but the point is not sufficiently important to the problem of learning to warrant the extended treatment that would be required to demonstrate it. Some interesting *Umweg* experiments might be made with potato sprouts in cellars.

Now Thorndike had the insight to recognize an ultimate fact when he saw it and did not attempt an 'explanation.' In the more than thirty years since his observation was first published it has never been explained, and I suggest that we proceed for a while on the assumption that it never will be. In discussing this valuable observation in the monograph previously quoted I wrote:

> As to the tendency for the act to diminish into a mere vestige and not to be immediately repeated if the door is not opened the first time: in the loop box the animal experiences something that resists, then gives way and more or less immediately thereafter, the release. (This may be the clatter of the falling door or the sight of the hole in the wall, or both.) Further, the release does not occur unless the other experience occurs. Now to the experimenter who releases the cat from the other box, a lick is a lick and a scratch is a scratch, no matter of what energy or what degree of attention by the animal. There is no irreducible minimum of effort as there is in pulling the loop. The animal achieves the same end with a little lick as with a big one, and behaviour is parsimonious ((1), p. 82).

That is all that can be said about it. Behaviour *is* parsimonious and there is nothing (at least for the present) that we can do about it. It may profitably be regarded as a fundamental property of animals that we must learn to use in the formulation of our problems. One manifestation of this property of parsimony has been recognized by a number of investigators for a long time, but always, so far as I know, in confusion with another property. This derivative fact, about economy upon repetition, has been given various names such as the 'law of least action' or of 'least effort' (45). Szymanski (36) called it '*das Prinzip der kürzesten Bahn.*' Gengerelli (15), who has recently supplied some good illustrations of it, calls it 'the principle of maxima and minima' and tries to make it do too much. His formulation, in addition to being the most recent, is one of the clearest that I know. It is as follows:

> The behaviour of an organism elicited by a given stimulating situation which affords relief to an internal need of that organism tends, *with repetition*[1], to approach, in terms of time, space and effort involved, the minimal limit compatible with the relief of that need; the nature of the limit being defined by the structure of the organism and of the external situation ((15), p. 232).

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[1] Italics mine. Several pages earlier ((15), p. 228) Gengerelli says, in italics, that "*the organism, under the stress of a need, tends in consequence of repetition, to relieve that need by the process of least effort.*"

This, as a generalization of observed fact, can hardly be challenged, for it is corroborated by every learning curve ever drawn. Gengerelli considers it self-evident enough and all-embracing enough to be taken as one of those facts "which do not stand in need of explanation," to serve as the "cornerstone of our explanation" of learning. Its self-evidence is not likely to be questioned, but whether it is "capable of embracing all of the varied phenomena with which we have to deal" is another matter. Perhaps it would be useful to sacrifice a little self-evidence for greater extension. Certainly any candidate for a postulate should have some implications beyond the phenomena it is designed to 'explain.'

What has 'repetition' to do with this matter? We have seen that it is primarily merely a means of detecting economy. Its inclusion in the above formulation probably derives from the notion that repetition is a causal factor in learning, and inasmuch as this postulate is not required to do more than explain learning (which is apparently the phenomenon described by the postulate itself!), it embodies that traditional misconception. That there is no causal relation between 'repetition' and learning is clear from the fact that each may occur in the absence of the other. Learning occurs without repetition and repetition occurs without learning (39). The relation between them is accidental and wholly contingent upon a variety of special conditions, some of which we shall have later to mention.

If, instead, we consider that *all* behaviour is parsimonious, as I have already proposed, we shall have an instrument of considerably wider utility. Accordingly, I shall consider parsimony a property of the class animals, co-ordinate with needs and the others to be discussed. It may be described briefly as follows: animals approach a needed object or state of affairs by the shortest route or most economical means functionally possible. Now 'functionally possible' may be taken to mean "possible for that particular animal at that particular time (*i.e.* point in its history or experience, physical condition, etc.) in that particular situation." It is thus differentiated both from the 'physically' possible and from the potentially possible for that animal. (This qualification is made only for temporary convenience of exposition and loses its significance when the matter is properly regarded.) Now this property of parsimony is not so 'self-evident' as the 'principle of maxima and minima.' It can be observed most clearly in situations and under conditions (inside and outside the animal) such that what I have called the functional and the physical routes of least roundaboutness coincide[1]. This occurs under certain conditions when only one need is involved as in certain 'tropisms' (cf. (9)), where the structural or physiological factors that limit parsimony are usually fairly evident. In much the same way surface tension can be observed most clearly under certain special conditions, *i.e.* when a drop of one fluid is suspended in a quantity of another of like specific gravity. But there is no reason to suppose that parsimony ceases to hold when more needs than one are involved, as is usually the case in psychology. Physical laws can be verified only in specially constructed 'pure' cases, but we do not suppose that they lose their validity in the everyday world.

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[1] Cf. Bingham's 'principle of the readiest means,' (4), p. 25, and (5), p. 3.

The recognition of parsimony as a property of animals (organisms) thus substitutes for the sterile question, "Why, or how, does the 'correct response' get selected?" the much more profitable and answerable one, "Why is it (the *potentially* most economical) not always selected at once?" The behaviour of the paramoecium bumping his way round an obstruction does not look very parsimonious until we consider his peculiar structure. In the case of Gengerelli's rats, for example, we must ask why they did not take the diagonal—the physically and potentially shortest—path at once. Part of the answer is that the animals were satisfying different needs at different times. The naïve assumption that they were always satisfying the need for food is entirely unwarranted and a direct consequence of insufficient and insufficiently critical anthropomorphism. Thus, at first, the field was largely determined by the need to get information (Pavlov's 'exploratory reflex'). Later it was jointly determined by the need for food and the need to stick to the wall ('stereotropism'), and eventually by the need for food (practically) alone. If the other needs had been satisfied first, it would have been evident that repetition has nothing to do with the property of parsimony. But here again the confusion of this property with the learning process has vitiated both the experiment and the 'postulate.'

We find then that parsimony is a universal property of the class animals as surface tension is a property of the class liquids; that *one* manifestation of this property is economy upon repetition of an act; that this particular manifestation of the property of parsimony is often used as a behaviour sign or even as a 'measure' of learning; that this sign is practically always mistaken for the process of learning itself. It will be apparent by now that the 'law of least action' involves a confusion between the property of parsimony here described and the next property to be mentioned, *i.e.* retentivity.

What then becomes of the 'law' of exercise or repetition or frequency? We have seen that parsimony is a property of the class animals, comparable to growth. Now growth takes place in time and requires time to become evident. Economy (usually) requires repetition to become evident. But how far would we get in understanding the biology of growth if we solemnly proclaimed that the sole law of growth is that size increases, within limits, with time? Yet that is essentially what the 'law' of repetition or frequency asserts with regard to the psychology of learning: *i.e.* that learning or efficiency, within limits, increases with frequency of repetition[1]. In either case, a condition necessary to, or useful in, the detection of a process is considered its cause. When the rôle of repetition as a rather clumsy but still very useful instrument for detecting the economy that is a sign of learning is once clearly seen, the point requires no further elaboration, and the 'law' of repetition is seen to be meaningless.

The familiar fact of insight *alone* is sufficient to demonstrate once and for all that there is no real or necessary or causal relation between learning and repetition or frequency of repetition. Whatever else insight may or may not be, it is a kind of learning which does not require repetition in order that its expression should attain maximal economy. In spite of its familiarity we managed, with sufficient determination, consistently to ignore[2] the fact of insight in our thinking about learning until Köhler (24) forced it upon our attention by exhibiting it in apes. Even then there was, and is, an uncomfortable feeling that a fact so disturbing to our revered 'laws' of learning ought either to be argued out of existence or to be repealed by edict of the Behaviourist[3]. One thing at least is finally established by the mere occurrence of insight, and that is that learning is *not* caused by repetition. Certain special conditions under which repetition may seem upon superficial examination to have a causal relation to learning will be discussed in a later section.

(c) *Retentivity.*

Another property of organisms, more generally recognized than parsimony, and consequently requiring less extended treatment, is *retentivity*. This has been called by various names, *e.g.* Semon's (33) *Mneme*. One of its particular manifestations is called 'memory.' Now for some obscure reason this *property* of retentivity has, like that of parsimony, usually been confused with the *process* of learning. Perhaps this confusion may be traced to the fact that the detection of retentivity always, and the detection of learning often, requires repetition.

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[1] It is not necessary here to criticize particular formulations of this 'law.' These considerations are believed to apply to the idea in all its varied expressions.

[2] Or else to throw it into the catch-all of 'meaning,' another problem in urgent need of restatement.

[3] Or the Anthroponomist ((18), pp. 575–581).

The physiological or physico-chemical basis of this property is naturally a matter of very great interest to psychologists: but it is not primarily a psychological question. We do not need, and cannot afford, to wait for the chemist's explanation (if indeed one ever become possible) in order to recognize the property and its importance to psychology. Certain animal protoplasms (nervous) or organizations thereof seem to possess the property of retentivity in greater degree than others. That also is interesting and important, but the more essential fact is that it is a fundamental property of the class animals, entirely co-ordinate with and distinct from the previously described property of parsimony. The fact that both of these may be displayed by means of repetition does not in the least affect their mutual independence.

The 'laws' of disuse and of recency are expressions of the fact that this retention has temporal limits. They do not deal with the learning process at all.

This property, like all those mentioned, has no necessary relation to consciousness. It may or may not be manifested by conscious processes.

(*d*) *Sensitivity.*

The fourth and, for the present purpose, the last of the general properties of organisms that requires to be mentioned is that of *sensitivity*. It too is generally recognized for what it is (namely a *property* of organisms co-ordinate with needs, parsimony and retentivity) thanks largely to the insight of Verworn (41), who called it 'irritability.'

The property of sensitivity may be briefly characterized as that in virtue of which an organism acquires information about its environment. Now this 'information' may be anything, from that received by a paramoecium when it enters a drop of acid to that received by a physicist when he reads a differential equation. The property of sensitivity, like all the others here discussed, has no general or necessary relation to consciousness. The 'information' received by an organism in virtue of this property may or may not be represented in consciousness by sensations, perceptions, or ideas.

III. ON THE NATURE OF THE LEARNING PROCESS.

Before defining the nature of the learning process it is desirable to inquire what are the necessary and sufficient conditions of learning. To this end it will be useful to refer again to the previously quoted general description of adaptive behaviour in very diverse situations (p. 152 above). It contains a serious possibility of confusion in the third proposition, which states that the situation may be so disposed as "to facilitate or to impede the cat's getting what it wants." These two possibilities are by no means co-ordinate. 'Facilitating' factors may indeed be present in the *situation*, *i.e.* among the 'other things' than the cat and what it wants. In some experimental situations they constitute very important factors indeed. But they are usually at the outset 'facilitating' factors *only for the experimenter and not for the animal.*

This brings us to a far-reaching and vitally important distinction that I have elsewhere discussed at considerable length in criticizing Thorndike's neglect of it ((1), pp. 75 ff.). This is the distinction between the *situation* and what I shall call the *field*. By 'situation' is meant the physical environment, the 'other things' of the quoted description. The 'field' is the *real* situation in the customary non-psychological use of the word: it is the situation *as it is for the animal.* Again, the field is the situation as it *is* (has existence) for the animal, as it is 'sensed' or 'received' or 'recepted' (must I repeat, without any implication of consciousness?) by the animal by virtue of its property of sensitivity (as well as *all* the other properties we have described). Factors in the *field* may be anything, from an amoeba that has just escaped from another amoeba, to prohibition, economic depression and delusions of persecution.

With the distinction between situation and field clearly in mind it is evident that while the *situation* may contain both facilitating and impeding factors, the *field* of an animal 'in' that situation may contain (of these two) only the impediment. The two types of factors are *not*, as their collocation in the quoted proposition might suggest, co-ordinate.

The presence in the field of an impediment, or obstruction, or obstacle, constitutes a necessary condition of learning in general, for obvious reasons. If there is no obstruction in the field learning cannot occur, because the need is satisfied with maximal potential parsimony at once.

The obstruction may be anything, from a barbed-wire fence when the need is to pick up a shot grouse on the other side, to the weight of tradition when the need is to reformulate a scientific problem. The impediment in the field may coincide with a physical barrier in the situation, as in some *détour* (*Umweg*) situations (which incidentally present the simplest possible concrete realization of the conditions of learning), or it may have no counterpart at all in the situation.

The next step is obvious: given the four described properties of organisms, a need, parsimony, retentivity and sensitivity, the presence of an obstruction in the field is the sole and sufficient, as well as the necessary, condition of learning.

The 'law' of effect will now be seen to contain (beside its implications for retentivity the propositions that in all cases of learning there is a need and there is an obstacle to its satisfaction. It thus makes considerably more sense than some of the other 'laws.' The fact that more or less irrelevant 'punishments' and 'rewards' are sometimes efficacious is due to the particular (and more complicated) structure of the *field* in those instances.

Discerning psychologists have seen for a long time that learning is a species of adaptation. Yerkes (46–49) has for years been calling learning 'adaptive behaviour.' A behaviouristic colleague once criticized the use of this term by Yerkes and the writer, by saying that if adaptive meant anything *all* behaviour was adaptive.

Adaptation certainly means something very concrete and important. It is a collective name for the class of all those processes which are conditioned by any need, specific or individual, of any animal. It is, so to speak, what gets done about a need. Adaptive behaviour is any behaviour conditioned by any need of any animal.

This conception of adaptation and adaptive behaviour obviously is more inclusive than the notion that they mean a class of events or behaviour that is in some way useful or beneficial or pleasant to the animal. It is equally devoid of any implication of value, teleology or hedonism. As McDougall (29) has pointed out in criticism of hedonism, many specific needs, notably those of a female with young, are directly opposed to the pleasure or even to the survival of the animal. The satisfaction of a neurotic compulsion may be accompanied by no pleasure whatever, yet it is a real if only an individual need.

Satisfaction of a need does not at all imply a subjective satisfaction, or pleasantness, or pleasure. It should be repeated that needs have no general relation to consciousness. Whether a need, in any particular case, has a conscious correlate is relatively unimportant.

My behaviourist critic was right, though he did not believe it, when he said that *all* behaviour was adaptive. All behaviour is conditioned by a need, or by needs. It might seem at first that an exception should be made in the case of diffuse discharge such as emotional behaviour, laughter and restless behaviour, but this is not the case. A behaviourist might urge that an exception should be made in the case of the reflexes, but this also is not the case. At the time of the conversation mentioned (1927) I too thought it necessary to distinguish two co-ordinate sorts of behaviour, *responses* and *acts*, the latter alone being characterized by reference to a need. They may perhaps still be usefully distinguished, but not on that basis. Responses (reflexes, motor habits, etc.) are always either one of two things. They may be acts in themselves, that is, they may be the functionally most parsimonious means to the satisfaction of a particular need, *e.g.* pupillary, wink, or scratch reflexes, etc.[1] Or they may be integral and dependent *parts* of acts. In either case the units of all behaviour are *acts*, and these are *always* determined by a need or needs. (Cf. (13).)

As previously noted, a few psychologists have known for a long time that learning is a species of adaptation. If we recall the definition of adaptation and the property of parsimony it is evident that this is the case. So far as I know, however, the exact nature of the relation, the differentia of the species learning in the genus adaptation have never been clearly stated. They are as follows.

Adaptation has two species. In other words, the processes conditioned by any need of any animal may be of two kinds: (1) changes in the need itself, and (2) changes in the field. It is the second of these that is usually called learning. (Cf. (21).)

This may become clearer through a consideration of the type of learning which, since Köhler, is generally called *insight*. Now insight possesses the extremely valuable characteristic of occurring sufficiently rapidly to permit us to see the true nature of the process of which it is an extreme case. Some processes in nature occur too rapidly to be seen and some occur too slowly. Learning is one of the latter. It is quite possible that if it required a much longer time than it does for the signs of surface tension to become visible the physicists might have been led to ask all manner of foolish questions about it. Learning usually, and always under the conditions customarily established in experiments on the subject (*i.e.* puzzle boxes, nonsense syllables, and particularly mazes), occurs too slowly for its real nature to be evident[2].

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[1] It is perhaps necessary here again to remind the reader of the truism that a need does not have to be conscious in order to be an effective determinant of behaviour. A moment's reflection will show that the foregoing applies equally well to such reflex responses as heart beat, which adjusts to meet the need for oxygen, etc., under such varied conditions as temporary violent exercise and permanent arteriosclerosis.

[2] There have been some very ingenious and valuable uses of the maze (in many of these cases it ceases to be a maze, *e.g.* (10), (15), but for the most part it has proved an unmitigated nuisance. It has filled the literature with elaborate investigations of pseudo-problems. It has provided an easy satisfaction for the need of many psychologists to do something 'experimental' without forcing them to set a real problem. Its use has become so ingrained and recourse to it so automatic that it short-circuits even conscientious attempts to

Thus behaviourists have even asserted that the conditioned reflex is the type of all learning, though they do not seem to have attempted the task of showing precisely *how* other types of learning, more properly so called, may be related to it. The fact is that the conditioned reflex is the one type of learning that manages, even more completely than maze learning, to conceal the true nature of the learning process in a mass of special conditions.

In the case of insight, however, the learning process occurs fast enough for its essential character to become clear. Inasmuch as the term has been somewhat abused in recent years it is perhaps desirable to adduce some specific instances. The case of Köhler's ape ((24), pp. 107 ff.) who, being in need of a stick, sees a branch of a tree for the first time as a stick, is an excellent one. In this case, an instant earlier the branches of the tree were merely something to be played with: he was sucking them as a matter of fact. His attention was drawn to the bananas outside the cage, and a need for the banana came into existence. With this animal, by reason of his earlier use of sticks, the primary need for the banana implied immediately a need for a stick. With the existence of this need the field was completely reorganized, and what a moment before was a tree became a treasury of sticks. In this case the reorganization of the field took place immediately upon the coming into existence of the need. (Incidentally, the need in turn did not arise until the animal was in a new field, that of the bananas. This instance thus exemplifies also the interdependence of needs and fields which is so characteristic of behaviour.)

It is in some respects still more illuminating to take a case in which the reorganization of the *field* is occasioned by an 'accidental' change in the *situation*. The following is one that can very easily be duplicated. A piece of liver is placed on a small pan in a large square cage, enclosed on three sides by wire mesh and on the fourth by a horizontal grill through which the pan can be pulled by the attached string. The string is laid in a loose right angle (cf. (1), p. 114, Fig. 15) so that about a metre of it must be pulled out before the pan will be moved by the pulling. A cat which has never pulled, played with, or even seen a string is admitted to the room. She soon finds the liver and claws energetically at the wire *nearest* it. She persists in this for a while but eventually gives up: *i.e.* the need subsides with failure, other needs arise. In Freud's language, she suppresses the

formulate a problem with decent precision. Its 'reliability,' or lack of it, has furnished many opportunities for a flight from the difficulties of psychology into statistical virtuosity. It has paralysed or stultified current thinking about learning to an extent that we are not yet in a position even to estimate.

need: in Lewin's, which, though also designed for human problems, here fits admirably as a description of the behaviour, she "goes out of the field[1]." She may return from time to time to claw directly at the liver but eventually gives up for good. Under these circumstances, possibly as a 'substitute activity,' of which I have noted many instances ((1), p. 92), she may play with the string. In the course of this play the string is eventually pulled straight and the next pull moves the pan. This 'accidental' (*i.e.* occurring in the course of activity determined by a different need) change in the situation, and ultimately in the field (the cat hears the pan move and feels its inertia), brings about a still greater one. The need for the liver, which was in abeyance or non-existent, recurs, and the string is no longer a plaything but something that brings the liver nearer. The cat goes to the cage, this time where the string comes out; not (as before) at the point *physically* nearest the desired object but at that which is now *functionally* nearest. She pulls the string again, this time with intent and with gaze fixed upon the pan. She may pull it again, watching instead her foot and the near end of the string. Then she pulls out the liver without further ado.

It is probably not necessary to mention other instances of insight to make the point clear. Though the particular reorganizations of the field involved will vary tremendously, some sort of reorganization occurs in every instance of insight. In the *détour* or *Umweg* problems which, as we have seen, present the simplest possible realization of the conditions of learning, the reorganization consists in the metamorphosis of the first part of the *détour* from a 'going away' into an integral part of a 'going toward.'

There will probably be no serious objection to the proposition that insight consists in a (rapid) reorganization of the field, since that may fairly be called self-evident. It is not at first glance so obvious that *all* learning consists in the reorganization of a field, since in many cases the process occurs very slowly. It is possible in every case, however, to show that the 'end-product' of the learning process is a *reorganized* field, and in many cases to determine the nature of the reorganization at once. Where the latter is not at once evident, there is a real problem for experiment. But before considering these problems in greater detail it is necessary to note the conditions, beyond the obvious ones of the situation, that determine fields in general.

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[1] This is equally true when the cat in the puzzle box stops trying to get out and washes and purrs contentedly. While still physically 'in the puzzle box' it is no longer there psychologically ((1), p. 92).

We have seen that animals have four properties of importance to learning: needs, parsimony, retentivity and sensitivity. Sensitivity may be very highly differentiated, as in man and the higher animals, or it may be very slightly differentiated, as in the protozoa. The field will naturally be determined in part by the sensitivity of the animal. Visual objects do not occur in the field of an animal without photoreceptors.

The constitution of the field depends also upon retentivity, which likewise varies greatly in different animals. To an animal that has once used a stick as a tool, sticks may retain their tool character for a considerable time. Within this time a situation containing sticks presents quite another field than that which the animal has in the same situation before he has used sticks and after they have lost their tool character.

Parsimony and needs may be considered together. The ultimate dynamic factors in all fields are the needs. Parsimony determines the direction of the movements toward the needed object. Where in other respects equal possibilities for various fields exist, the need that is strongest will determine which potential field is realized[1]. On the other hand, as we have seen, an 'accidental' change in the field may occasion a new need.

These four properties are conditions of all fields. The particular type of field which gets reorganized, the type, that is, with which learning is concerned, has one additional condition, namely, an *obstruction* to the satisfaction of the need, as we have already seen. Now in all fields containing an obstruction there occurs a phenomenon of paramount importance, namely, that an obstructed need *distorts* the field in which it occurs to an extent proportional to the strength of the need and the completeness of the obstruction. The precise determination of the kinds of this distortion and of the conditions under which each occurs constitutes, in my opinion, the fundamental task of the psychology of learning. At present we know practically nothing about it. I have a little evidence that, under the influence of a very strong and completely obstructed need, the field contracts spatially ((1), p. 141). I should be inclined to guess that, under certain conditions, it would also contract temporally, but that again can only be settled by experiment. We shall have occasion later to make several other hypotheses about the varieties of distortion which an obstructed need may produce in a field.

We may be entirely certain of only one thing: that *some* kind of distortion of the field is caused by obstructed needs. The most striking example of this occurs in man in autistic thinking. Here the *field* is so distorted by a strong and frustrated need (self-assertion, sex, or what not) that it no longer bears any resemblance to the *situation*. (Such a field is still *determined* in part by the situation, *e.g.* by the type of obstruction it contains, even when it least resembles the latter.) Somewhat less remote from the 'real' situation, but still diverging materially from it, is the field in what is known as 'wishful thinking.'

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[1] In the conscious fields of humans and the higher animals this fact is called 'attention.'

In general, of course, imaginal or ideational fields offer much less resistance to distortion, are much more plastic and pliable than other fields, and consequently furnish the most extreme examples of distortion. The manifestations in consciousness of this distortion of the field may also be observed in *perceptual* fields. One of the most valuable of the observations reported in Köhler's book on apes was made upon himself. The following is his description:

> Even before the chimpanzee has happened on the use of sticks, etc., one expects him to do so. When he is occupied energetically but, so far, without success, in overcoming the critical distance, anxiety causes one's view of the field of action [what is here called the situation] to suffer a phenomenological change. Long-shaped and movable objects are no longer beheld with strict and static impartiality, but always in a 'vector' as if with a drive toward the critical point ((24), p. 36).

We constantly see the same and other varieties of distortion of perceptual fields by obstructed needs in everyday life. The impatient motorist for whom red is green, the anxious hunter for whom any moving object in the brush becomes a buck, and countless other instances indicate the universality and versatility of distortion in perceptually present fields.

It must not be supposed that *conscious* fields alone are distorted by needs. They simply provide the clearest and most familiar examples. The distortion of the field of a tree in a dense forest by its need for light is reflected in the distortion of the tree itself. Isolated, it would branch and spread. When the light is obstructed by other trees, it becomes a straight bare trunk with a few branches at the top. The particular mechanism of any such phenomenon (*e.g.* 'selection,' the fact that collateral branches do bud and die) must not be allowed to obscure the fact that the field is distorted by an obstructed need.

As has been noted, given the properties of organisms, the sole additional condition of learning is an obstruction. An obstructed need always distorts the field. The determination of the varieties of this distortion and of the conditions under which each occurs constitutes the proper task of the experimental psychology of learning. Learning is the process of reorganization sometimes undergone by fields distorted by an obstructed need. When in such fields learning does not occur, the other sort of adaptation (change in the need) must occur.

IV. VARIETIES OF ADAPTATION.

(*a*) *Changes in the need.*

For the present purpose this group of problems does not require extended treatment. The dynamics of needs in general are being very successfully investigated by Lewin and his collaborators (28).

We have here only to mention some varieties of changes in needs that have a more or less intimate and obvious relation to the general (*i.e.* not specifically human) problems of learning.

One of the most striking changes that needs undergo is that consequent upon their satisfaction or (under compulsion) supersatiation. Dodge (11, 12) has dealt with a related phenomenon in conjunction with the refractory phase of nervous tissue, and the two phenomena may be very closely related indeed. Karsten (20) has made the most extensive study of satiation. The change undergone by a need in this process amounts to a complete transformation. The need for food may be satisfied to such an extent that it becomes (or there occurs) a need to get away from food even to the point of regurgitation. Incidentally it is here that, for the first time, we find repetition really functioning in a psychological process. It *does* produce satiation (indifference) and, under compulsion, supersatiation (repulsion), which is a complete transformation of the need.

An extreme case of this is the familiar one of the new operative in the chocolate factory or the new girl in the candy shop. According to tradition the needs of these people for sweets are quickly supersatiated to such a degree that they *never* recur. But we need not depend on tradition. It was this very phenomenon, the only one of which repetition may properly be considered a cause, that led Dunlap (14) to question the 'law' of repetition. The cases reported by him and by Holsopple and Vanouse (17) of correcting a constant typing error by 'practising' the error are cases of the supersatiation, under compulsion, of a weak lower-order need which was already in conflict with a strong higher-order need. By a first-order need I mean the need that determines the whole field. By a lower-order need I mean a need that occurs only in a field determined by a another need. Thus Dunlap's need to spell 'the' 'hte' occurred only in the field determined by the need to type something intelligible. I think this is probably a more useful distinction at least for our purpose, than Lewin's between '*echte*' and '*quasi-Bedürfnisse*.' Thus Dunlap's 'gamma' hypothesis is more nearly correct than the 'beta' hypothesis, and it is not a matter of 'probability.'

A second way in which needs get changed occurs when an obstruction makes their satisfaction functionally impossible. The Freudians call this *compensation*, but it might more properly be called *substitution*. Its mechanism is not clearly understood, but the phenomenon consists essentially in the fact that when the satisfaction of a strong need is *completely* obstructed, another need becomes still stronger. The only principle of selection of this substitute need that I have been able to observe is the obvious one that the substituted need must be one for which satisfaction is relatively *unobstructed.* The cats, for example, that failed entirely to escape from the puzzle boxes ((1), p. 92) generally washed themselves most assiduously, as if nothing else in the world had any importance. This substituted need may eventually be so well satisfied that they will lie in their cages and purr.

A phenomenon very frequent in human beings but also occurring in many other animals is that of the *induction* of needs. A need may occur in an animal upon the occurrence within its field of the satisfaction or approach to satisfaction of a like need in another animal. The Smiths need a new car, though their old one is in excellent condition, because the Joneses have purchased one. The ape ((24), pp. 35, 64) that was quite indifferent to the banana outside his cage needs it so badly when another ape approaches it that he invents a method of getting it. Kuo (26) has recently reported some very painstaking experiments, on the genesis of rat killing in cats, which gain measurably in significance when regarded as experiments on the induction of needs, and which should be extended with this question in mind.

(*b*) *Changes in the field: learning.*

Any doubts that *all* learning is a reorganization of a field determined by an obstructed need may be dispelled, even in those cases where the process itself is too slow to be observed, by examining the 'end-product' of the process and comparing it with the original field. The keyboard of a typewriter presents quite another field to the experienced typist than it did when she first made its acquaintance. The maze is very different to the rat after he has been through it. The buzzer has a wholly new place in the field of the 'conditioned' dog. The 'end-product' of all learning, even in the technological sense, is always a reorganized field.

These examples are of the kind that behaviourism would bring forward in objection to the proposition that repetition has no causal relation to learning. We must therefore consider why, in these and similar instances, repetition may appear to possess great importance.

We may choose typewriting as the representative of the whole group of skills, because the *situation*, the 'physical' correlate of the field, so to speak, is more obvious and concrete in this instance than in most of the others, and the exposition will consequently be clearer to those who insist upon a visible physical correlate for the field. In the higher animals the property of sensitivity is developed to an extraordinary degree in certain tissues, but in each for only one particular kind of physical event, *e.g.* light waves, periodic pressures, aperiodic pressures, etc. Now some of these tissues, especially the eye, *including* its phylogenetically more recent parts in the central nervous system, yield, for reasons into which we need not here inquire, a very wide and highly organized field[1]. Others, notably the somaesthetic, including the kinaesthetic, yield in isolation relatively small and unorganized fields.

But there are other important differences among the various senses, failure to note which has contributed largely to the behaviouristic fallacy. Among these must be mentioned the fact that the adequate stimulus for some of these specialized tissues is movement, or strain, or pressure, as in kinaesthesis. In other words, there *is* no (perceptual) kinaesthetic field unless something is being done. In order merely to *produce* a kinaesthetic field something must be done. Now this 'doing' may be called a 'repetition,' but the fact remains that it is a necessary condition to the *existence* of a (perceptual) kinaesthetic field, just as opening the eyes is necessary for the existence of a visual (perceptual) field. Thus a 'repetition' has the same relation to a kinaesthetic field that a 'glance' has to a visual field. To say that it requires so many 'repetitions' for a kinaesthetic field[2] to be reorganized corresponds exactly to saying that it requires so many one sigma exposures for a visual field to be reorganized into something recognizable.

The important psychological fact that there is in either case an optimal temporal distance between successive doings or exposures ('repetitions') concerns only the property of *retentivity*, which varies in different animals, tissues in the same animal, etc. It has nothing whatever to do with the learning process as a scientific concept. This confusion of retention and learning, which is almost universal, is another factor that has militated against a proper understanding of the problem. Such facts as that mentioned do have some significance for 'learning' when the latter is conceived teleologically and anthropocentrically as a practical utilitarian, technological *improvement* in a desirable performance. It need hardly be pointed out that such concepts, however serviceable they may be in the various technologies that draw upon psychology, have no place in science. The behaviouristic attempt to make 'laws' about such matters is characteristic of the teleological bent of that doctrine.

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[1] It seems practically impossible in uninjured animals to produce a visual field without a considerable degree of organization.

[2] Throughout, the expressions are 'kinaesthetic field,' 'visual field,' etc., are used to mean *predominantly* kinaesthetic, visual, etc. Probably no field is entirely restricted to one modality in animals with differentiated sensitive tissues.

Neglect of this obvious difference in the type of stimulus adequate to the various sensory tissues (including always their central parts) has misled behaviourists (*e.g.* (18), pp. 575–581) into an attempt to argue the fact of insight out of existence because it does not occur in kinaesthetic fields. They do this in spite of the fact that they usually accept Stricker's (34) theory that human thought is generally a kinaesthetic field, and hence, in most people, the type of field in which insight most commonly occurs. Incidentally, it may be remarked that it is at least at present much more illuminating to ask, not why insight occurs, but *why it does not always occur.*

It should be noted also that kinaesthetic fields are usually more extended in time than are visual or auditory fields. This is in part a necessary consequence of their limitation in scope. To use again the example of typewriting, the operator has only ten fingers and some thirty or forty keys. She cannot touch all the keys at once. Similar considerations hold for all skills. This fact of greater temporal extent combined with less 'spatial' extent of kinaesthetic fields has doubtless contributed to the confusion inherent in the notion that repetition has a causal relation to learning. The matter is further complicated by the fact that the reorganization undergone by a kinaesthetic field while and after it runs its temporal course can usually be *detected* only upon repetition.

If now, to recapitulate, we ask why repetition has ever been thought to have anything directly to do with learning we must answer: (1) The learning *process* has been confused with the *properties* of sensitivity and retentivity. (2) Attention has been confined to learning under such conditions that it occurs too slowly to be evident without some such indirect sign as economy upon repetition. (3) Consideration has been limited largely to kinaesthetic learning, *i.e.* to the reorganization of kinaesthetic fields, in which, owing to the peculiar nature of the adequate stimulus, namely, 'doing,' phenomena which in other senses would be called exposures are called repetitions.

These considerations obviously apply with equal force to learning in the maze. In this case, however, the possibility of other than a kinaesthetic field is more or less excluded by means of walls, which are made as visually homogeneous as possible, and by various other measures. Similar considerations, which will be evident to the reader, apply to nonsense syllables and to rote memorizing.

The special conditions which might give repetition the appearance of a causal factor in the learning of multiple choice problems will be discussed in a later paper reporting some hitherto unpublished experiments with cats.

Somewhat similar conditions obtain in the case of discrimination habits in animals. Every experimenter knows that in the establishment of such habits the battle is won as soon as the objects to be discriminated become parts of a single structure (22). In some recent experiments (soon to be published) on weight discrimination in rats, the differences used were so large that the heavier weight obviously taxed the rat's strength to the limit while the lighter required very slight effort. In spite of these large physical differences, until the two were organized into one field, they were functionally equal. With human subjects we 'structure' the field from the start by means of verbal instructions. For other animals there is first an inductive problem to be solved.

The case, however, which most successfully conceals the nature of the learning process by means of *very* special conditions is that of the conditioned reflex. This important and interesting phenomenon has unfortunately been degraded by its enthusiastic devotees to the level of a catchword or a magic formula. It is indicative of the bankruptcy of the psychology of learning that the conditioned reflex could seriously be considered representative of learning in general and even hailed as the key to all its mysteries. The conditioned reflex is another of those types of learning that, unless analysed, seem to suggest a causal relation of some sort between repetition and learning, and here again the special conditions that permit of this appearance should be exhibited. To do this adequately would require much more space than is available, but some of these special conditions may be indicated.

Here as everywhere else the 'end-product' of learning is patently a reorganized field. The point at issue is how this reorganization comes about. The current 'laws' of learning state or imply that repetition *per se* does it. That this is not the case is established by the occurrence of one case of insight. How it does come about we do not know, but we can at least ask sensible questions about it and make hypotheses which may be tested.

It should be noted first that the field of the animal in the conditioned reflex situation is determined by a need. If the dog is not hungry there will be no salivary reflex. Second, it must, as in all learning, be an *obstructed* need. The to-be-conditioned stimulus must occur before the unconditioned or preconditioned stimulus. If it occur simultaneously with or after the latter, the need is no longer obstructed and there is consequently no dynamic factor to bring about a reorganization of the field[1]. Third, the need must, as always, be the strongest need of the organism at that time. "Thus, for example, if the dog has been so fastened up that anything causes severe irritation, it does not matter how many times the combination of stimuli is repeated, we shall not be able to obtain a conditioned reflex" ((31), p. 28). In such a case the 'stimuli' simply are not in the field. With the same physical energy as when under other conditions they are effective, they are not really stimuli at all. When the field is determined by the need to escape the irritation, instead of by the need for food, they have no existence.

That the field is determined by the strongest of several competing needs is even more clearly indicated by certain experiments reported by Pavlov ((31), p. 31). He found that it was possible to take a strong stimulus (*e.g.* electric shock) which evoked a strong unconditioned response of its own (*e.g.* the 'defence reflex') and make it a conditioned stimulus to the salivary reflex. "This, we consider, was due to the fact that the alimentary reflex is in such cases stronger than the defence reflex" ((31), p. 30). But if the shock were applied over bone, the defence reflex was stronger than the alimentary, and conditioning could not be obtained! "Successful transformation of the unconditioned stimulus for one reflex into the conditioned stimulus for another reflex can be brought about only when the former reflex is physiologically weaker and *biologically of less importance* [italics mine] than the latter" ((31), p. 30). This 'biological importance' is exactly what I mean by degree of intensity of a need[2].

"Conditioned reflexes are quite readily formed to stimuli to which the animal is more or less indifferent at the outset, although, strictly speaking, no stimulus within the animal's range of perception exists to which it would be absolutely indifferent" ((31), p. 29). Here again is recognition of the fact that the field is determined by a need or needs and that nothing exists in the field that has not some relation to some need, if it be only that which Pavlov calls the 'investigatory reflex.'

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[1] Beritoff (2) and Switzer (35) have supplied evidence that in the case of strong 'defence reflexes' with very small latencies an originally indifferent stimulus may be conditioned even when it follows the unconditioned stimulus by a very short interval. It is of course not surprising that a very strong need should continue to distort the field for a brief interval after its satisfaction.

[2] Pavlov's use of the term 'reflex' is so loose that the term had better be dropped for scientific purposes. He calls movements and activities *a* reflex (as *the* 'defence reflex,' *the* 'freedom reflex,' *the* 'investigatory reflex,' *the* 'reflex of self-preservation,' etc., which have nothing in common except the need which determines them. Both the 'stimulus' and the 'response' may be utterly different, as *e.g.*, in the 'investigatory reflex,' yet so long as the need is the same he considers them one reflex. Evidently he is really talking not about reflexes but about *needs*. Cf. also (30).

It should further be noted that conditioning is facilitated by the exclusion of other stimuli and apparently to the extent that they are excluded. The presumption is warranted by extrapolation that if this desideratum were completely attained (*i.e.* if there were only *one* event in the field prior to the unconditioned stimulus, which is impossible because of internal stimulation if for no other reason) one 'repetition' would suffice to establish a conditioned reflex. Pavlov reports many cases in which one was established in three to five trials, and at least one ((31), p. 27) which was set up in one trial. In general, it would seem that the more 'prominent' the to-be-conditioned stimulus in the field (whether this prominence be produced by the exclusion of other stimuli or by 'biological importance'), *so long as it does not arouse a stronger need* and hence create a new field, the more rapidly conditioning occurs.

Another very important circumstance is implied in the following: "Similarly, while with the help of a very strong[1] unconditioned stimulus it is possible to convert a very unsuitable stimulus—for example, one which naturally evokes a different unconditioned reflex—into a conditioned stimulus, it is exceedingly difficult or even impossible with the help of only a weak unconditioned stimulus to transform even a very favourable neutral stimulus into a conditioned stimulus" ((31), p. 31). This remark indicates that what the field lacks in structure must be compensated by the strength of the need. In other words, it requires a *very* strong obstructed need to distort the field sufficiently for two such otherwise irrelevant events as a buzzer noise and a piece of meat to form one object or structure. The only structuring factor in the typical conditioned reflex situation is temporal contiguity, which alone does not normally provide much structure, but which, when accentuated by a very strong need, is sufficient to make one object of two events that have nothing else in common.

We constantly observe the same thing in men when very strong needs are involved. The football coach whose team won an important game when he happened to have eaten macaroni (which he loathed) for luncheon, thereafter always eats it before a game. The fact that he detested it, *i.e.* that it 'naturally evoked a different unconditioned reflex,' merely gives it additional prominence and accounts for the game's having been won by the macaroni instead of by the tie he wore. The angler who lost his tobacco and had to smoke his friend's particularly bad brand throughout a most successful day thereafter buys that kind—but only when he goes fishing! In all of these, two events which have the same very slight structuring factor as the two stimuli in the conditioned reflex situation (*i.e.* contiguity in time) may become one object or figure if the field is distorted by a strong enough need.

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[1] A 'strong stimulus' here means a piece of meat when the dog is very hungry: a 'weak stimulus' means the same piece of meat when the dog is not so hungry. This is a good example, not only of the dependence of the field upon the needs, but of typical objectivist obscurantism. We have seen above that Pavlov usually calls a need a 'reflex,' and here we find him confusing it with 'stimulus.' It is by means of this sort of paralogism, which H. M. Johnson (19) has aptly termed the 'method of equivocance,' that behaviourism makes its specious appeal of simplicity and shirks the necessary analysis.

I am not clear as to when Pavlov considers a conditioned reflex established. Sometimes it seems as if it were when the new stimulus provokes about the same amount and rate of secretion as the original stimulus, but most of his conditioned reflexes never reach this point. In any case the amount of secretion that shall be considered indicative of a conditioned reflex is just as arbitrary as 'three errorless trials' in a maze or a grade of *C* in calculus. *All* of these reflect the technological or utilitarian concept of learning already mentioned. The endless and sterile discussions of 'criteria' of learning (*e.g.* in the maze) show how futile such concepts are in science. For learning in the true scientific sense occurs whether it is ever demonstrated by repetition or not. The field is actually undergoing reorganization from the first; whether in the particular 'favourable' direction desired by the experimenter or whether the field is the one that the teacher hopes it is, is quite irrelevant to the *psychological* problem.

We have seen that the only fields that get reorganized are those in which a need is obstructed, and we must make hypotheses about the nature of the distortion that brings about reorganization. Now while all fields are determined by *more or less* obstructed needs, not all are sufficiently obstructed for appreciable distortion to occur. Yet all fields are in greater or less degree organized. There must then be some factors of organization that are more or less independent of the needs, and determined primarily by the nature and organization of the sensitive tissues (central as well as peripheral).

There are such factors. Some of them have been known for two millennia and have been called 'laws of association' for two centuries. Some have been unearthed in the last two decades by the *Gestalt* psychologists who have called them *Gestaltfaktoren.* Most of them, both old and new, still require more precise characterization.

Owing to a curious and interesting combination of circumstances (the complete unravelling of which must be left to the historian), some of these principles, those called the laws of association, were long thought to be factors of *retentivity* only and not factors of *sensitivity*, of the organization of *present* sensory fields. This was partly because the latter was taken for granted[1], and partly because of too much anthropocentrism and insufficient (or, sometimes, insufficiently critical) anthropomorphism. At all events, this confusion is not now so prevalent, and the realization is becoming quite general that these factors of organization operate in all fields.

Now in most of the fields that occur in nature at least several and often all of the structural factors, contiguity (spatial and temporal), similarity, closure, continuity ("*objektive Einstellung*"), etc., are given in the situation. It is only or chiefly when man intervenes with artificial constraints that only *one* of these factors is present and operative. Thus, in the usual conditioned reflex situation the sole organizing factor is temporal contiguity[2]. This is, to be sure, not an unimportant one, but by itself it is probably one of the weakest. There is no other factor uniting the buzzer and the food in the conditioned reflex situation, but even so, if these are the *only* events in the field and the need is very strong, a conditioned reflex may be established in one 'repetition.'

The foregoing considerations do not by any means exhaust the special conditions that give repetition the appearance of a causal factor in 'conditioning' a 'reflex,' but the more important ones have been indicated. Others will be inferable from the discussion of those obtaining in skills. The general fact that repetition is used to provide a sign of reorganization (of both acts and fields) is here also an additional source of confusion, as is the inclusion of retentivity in the technological concept of learning as learning *enough*—enough for some practical human purpose.

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[1] In its relatively brief history psychology has exhibited a notable capacity for taking the wrong (*i.e.* the least useful) things for granted. General physiology escapes most of these difficulties and the necessity of clear definition by dealing only with (1) very constant needs, (2) stimuli which may be more or less purely physically defined, such as light, gravity, etc. (cf. (9)), and (3) sensitive tissues such that the field has a simple relation to one or two aspects of (4) a very simple situation. For these reasons it can get along with such crude and unanalysed concepts as that of tropisms.

[2] As Köhler ((25), p. 298) has pointed out, this does not need to be the case. The adequate and the inadequate stimuli might have any degree of organization from the very slight one customarily used to a quite irresistible unity. Investigation of the influence of 'objective' organization on the formation of conditioned reflexes should be very useful in the analysis of the structural factors. The only experiment I know of that approximates an attack on the problem, of which this would be a strategic special case, is that of Warden and Hamilton (42, 16). These investigators found that varying the factor of temporal contiguity had a marked influence upon the rate of reorganization of the field. Unfortunately they also varied other factors.

We have seen that repetition is incapable of bringing about the reorganization of a field, that it has no general causal relation to learning, and that in the absence of a need no amount of it would bring about learning. I have already suggested in discussing the conditioned reflex that the 'distortion' of a field by an obstructed need is what brings about this organization or reorganization. The hypothesis is now advanced more precisely and explicitly that *whatever structural factors* (contiguity in space and time, similarity, continuity, closure, etc.) *a field may contain are enhanced by an obstructed need in proportion to the strength (within limits) of the need and to the extent to which it is obstructed.*

Experiments to test this hypothesis with respect to certain of the structural factors are now in progress. Others will have to wait upon a clearer formulation of the factors themselves. Wertheimer (44) has made a bold and ingenious beginning upon this task, but there is still much necessary analysis to be done. Some of the factors as currently described seem to overlap others, and some—even Aristotle's similarity—need more precise characterization. To this extent the psychology of learning must wait upon the psychology of perception, although the investigation of the former along the lines here described may well help to clarify the latter. Perhaps Wertheimer's method (of opposing one factor with another) applied to animals would yield the cleanest analysis.

I have said that the fundamental problem of learning is the determination of the kinds of distortion undergone by a field in which a need is obstructed and of the conditions under which each kind occurs, and have advanced certain hypotheses. The validity of this statement of the problem of learning does not, of course, depend upon the correctness of the particular hypotheses here advanced. Further, the hypotheses themselves (that at least some of the varieties of distortion consist in the enhancement or accentuation of the given structural factors) do not imply that our present conceptions of the structural factors are either exhaustive or correct.

This formulation of the problem of course makes no pretensions to finality. More precise ones can and will be made, but they may have to wait for a changed intellectual climate. Perhaps the term 'learning' is so saturated with non-scientific connotations that it will ultimately be found useful to leave it entirely to technology, but we cannot hurry such matters. That some change is imperative in the type of question we ask about learning can hardly be denied. The utility of any proposed change in direction can be determined only by trying it.

V. SUMMARY.

Animals (and probably all organisms) have four properties which are of direct importance to learning: needs, parsimony of movement in their satisfaction, sensitivity and retentivity. 'Repetition' has only a contingent relation to learning. Some of the conditions under which it might seem to have a causal relation are given when: (1) the learning process is confused with the property of retentivity; (2) repetition is used to exhibit economy which, in turn, is used as a behaviour sign of learning; (3) consideration is restricted to fields (especially kinaesthetic or predominantly kinaesthetic) in which a 'repetition' is really an instantaneous exposure; (4) the field is for some reason temporally extended; (5) the situation is for some reason, usually the intervention of man, very weakly structured, *e.g.* presents only one or two of the structural factors which have been called the laws of association, the principles of grouping, etc., some of which possibly remain to be discovered, and all of which need more precise description.

Adaptation is what gets done about a need. It is of two kinds: the first, usually called adaptation, consists in a change in the need. This may take place in several ways, *e.g.* by satiation or supersatiation, by substitution, by induction, etc. The second kind of adaptation is called learning, and it consists in the organization or reorganization of a field. A field is a situation as it biologically *is* for a specific animal at a specific time. All fields are determined by needs and contain a functional minimum of entities. An obstructed need *distorts the field* (thus facilitating reorganization) in proportion to the strength of the need and to the completeness of the obstruction. The fundamental task of the experimental psychology of learning is the determination of the kinds of this distortion and the conditions under which each occurs. The hypothesis is made that at least some of the ways in which fields are distorted by obstructed needs consist in the enhancement or accentuation of the structural factors given in the situation.

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PUBLICATIONS RECENTLY RECEIVED.

Contemporary Schools of Psychology. By ROBERT S. WOODWORTH. New York: The Ronald Press Company. 1931. Pp. vi + 232. $2.50.

The schools dealt with are: Introspective Psychology and the Existential School; Behaviourism; Gestalt Psychology; Psycho-analysis; and Purposivism. Further rather scattered notes are added about prominent psychologists who stand outside all 'schools.' Woodworth himself tends to what he calls a "middle of the road" psychology. The book is a valuable one. Woodworth is a sympathetic and thoroughly competent expositor, and his critical remarks are almost always, as would be expected, sound and illuminating. These lectures should be widely read by students of psychology in this country.

Behaviourism. By JOHN B. WATSON. London: Kegan Paul. 1931. Pp. xi + 308. 12*s.* 6*d.* net.

Every psychologist who is interested in the scientific development of his subject will welcome the second edition of this attractive and challenging book. As compared with the first issue this volume is both more complete and less of an 'appeal to the gallery.' The treatment is still dogmatic, cocksure and very sweeping in its claims. But all these characteristics give the book a live and rousing quality which is very rare among psychological texts. More than ever a good student who already possesses some background of general scientific knowledge and some critical capacity may well be started on his way by reading first McDougall's *Outlines* and then this book, or first this book and then McDougall's *Outlines*. A large number of new observations appears in this second edition, which is a striking demonstration of the fact that Behaviourism remains a force to be reckoned with in modern psychology.

Das Gefühl. Von ARTHUR WRESCHNER. Leipzig: Quelle und Meyer. 1930. S. 204. RM. 7.60.

Prof. Wreschner has written a valuable general treatise on the character, conditions and functions of affect. A brief historical introduction is followed by three longer sections dealing respectively with the general characteristics of feeling and their accepted methods of study, with special types of affect (*e.g.* sensorial feeling, aesthetic feeling, feeling in relation to perception and idea, etc.), and with theories of affect. Of the theories dealt with, the physiological is most completely considered, but logical and psychological theories also come in for expository and critical discussion. As is often the case in a German monograph, there is slight evidence of any acquaintance with recent work published in other languages. In the physiological section, for example, there is no reference to Cannon, or to any of the researches into thalamic functions; in the psychological section there is no reference to McDougall or any other recent English or American or French writer. Nevertheless, the essay is a competent and interesting survey, based not upon original experiment but upon a study of the field which, within the limits set, is both thorough and judicious.

Psychologie. Von ALOIS HÖFLER. Herausgegeben und mit Anmerkungen versehen von ALOYS WENZL. I. Band. Leipzig. 1930. S. xv + 642. RM. 36.

This is a second and very greatly enlarged edition of a work that was first published more than thirty years ago. The change of emphasis in the two books is very interesting. In the first the author was particularly anxious to guard against the worst pitfalls of associationism; in this he is equally anxious to give analytical methods a fair field and perhaps a little favour. The bulk of this first volume is concerned with rather long general discussions. The main topics dealt with in the first part, after a general introduction which treats of the standpoint, task and methods of psychology, are: (1) fundamental psychological processes considered in relation to systematic psychology: four are distinguished: perception, judgment, feeling and tendency or desire (Begehrungen); and (2) the mind-body relationship. In the second part comes a discussion of hearing, vision, taste and smell, and kinaesthetic sensations, and a consideration of sensory problems in general. This is followed by a discussion of image processes and memory.

For the ordinary student there is too much in this book. Many of the most interesting remarks—of which there are no small number—are contained in the frequent and long footnotes, and in the appendix for which the present editor is responsible. These are largely concerned with the discussion of current researches and controversial questions. The influence of Ehrenfels and Meinong is apparent throughout the book. Certainly this volume should be in any good psychological library and should be used for purposes of reference. As a text-book for any but the very advanced and judicious student it is hardly to be recommended.

The Subjective Character of Cognition. By R. B. CATTELL. *British J. of Psychol. Monog. Supplements*, XIV. Cambridge University Press. 1930. Pp. viii + 166. 12*s*. 6*d*. net.

When any stimulus is presented to a human subject is there immediate reaction of a definitely perceptual order, or is there a primary subjective 'mental state' which develops later the characteristics of perception proper? This, broadly speaking, is the question which Dr Cattell endeavours to answer by his patient and elaborate experimental research. In this monograph he gives a clear and very full account of his methods, their results, and the significance which he believes his results to have in relation to psychological problems in general. The monograph is an important contribution to some little investigated and very difficult psychological problems. Naturally the conclusions are not free from some amount of speculation, but they are stated very fairly, and the evidence upon which they are based is always set forth in such a way that a reader has the fullest opportunity to exercise his own judgment.

The Relative Effects of Size of Type, Leading and Context. By HOWARD T. HOVDE. Philadelphia: Reprinted from the *Journal of Applied Psychology*, XIII, No. 6, December, 1929.

This careful and thorough piece of experimental work showed that the rates of reading of three hundred adult subjects were determined by the nature of the context of the material read rather than by the size and leading of the printed type; thus the reader read most of what interested him most. The readers usually expressed a preference for the larger type and leading, but this was probably due to their impression that more material had been read because more space had been covered; while their dislike of small type resulted from the increased attention necessitated by the crowded sensory units.

La Répression Mentale. Par PAUL SOLLIER. Paris: Félix Alcan. 1930. Pp. ii + 218. 15 fr.

Repression is defined in the widest possible sense: Whenever mental activity tries to find manifestation, expression or realization in any direction or in any form whatsoever, and at the time meets with resistance which prevents such manifestation, expression or realization, there is 'mental repression.' The conditions, types and effects of mental repression are all studied in some detail, though for the most part in general terms. A final "Essai d'une Interprétation Dynamique" turns largely upon physical analogies, on the strength of which Prof. Sollier is led to regard mental activity as "un phénomène d'ordre physique." The discussion is throughout very clear and definite and has a value which does not depend on the validity of the final conclusion.

L'Attention. Par REYNAX. Paris: Éditions Spes. 1930. Pp. vii + 69. 4 fr.

The author is convinced that no true understanding of human reactions can be attained along materialist lines. The clearest demonstration of this, he thinks, is to be found in a study of the types and functions of attention, the latter being regarded as a definitely psychical process. In this brief essay he tries to show just what attention can do and what its functional disorders carry in their train. Although it is in many ways interesting, this little book suffers (*a*) from trying to cover rather too much ground; (*b*) from leaving the whole notion of attention very undefined, and (*c*) from too great generality in its treatment.

Le Mensonge et la Caractère. Par RENÉ LE SENNE. Paris: Félix Alcan. 1930. Pp. 348. 45 fr.

A lie is defined as an intentional alteration of the truth. 'The truth' is naturally here taken strictly in its psychological sense to indicate what is believed or known to be true by somebody capable of distinguishing a difference between the true and the false. The author considers that the general condition of the lie is the temporary dominance of the whole personality by some one or some group of its constituent tendencies. Hence he enters upon a prolonged study of the capacity for dominance of various human tendencies. His study is not original in an experimental sense, but relies upon the classified results of experiments and investigations carried out by others, and in particular upon the work of Heymans and Wiersma. The book is somewhat too long, but it contains a great mass of interesting data and some shrewd theorizing. The sociological significance of the investigation is kept well to the fore.

Le Devoir. Par RENÉ LE SENNE. Paris: Félix Alcan. 1930. Pp. 604. 70 fr.

In this large and consistently clever volume psychology is often in danger of being pushed out of the way by philosophy. It appears that contradiction and its resolution are the soul of all development. Somehow moral obligation or duty, which is a universal experience, is an attempt to get over all contradiction. The various ways in which this attempt is made the author endeavours to describe with impressive ingenuity and at great length.

L'Année Psychologique, 1929. Edited by HENRI PIÉRON. Paris: Félix Alcan. 1930. Vol. I, pp. xvii + 464; Vol. II, pp. 465–936. 120 fr.

The original papers in this issue are: *A study of cutaneous pain*, by Henri Piéron; *Local associations and the fixation of images*, by Marcel Foucault; *A test of imagination carried out on Paris school children*, by A. G. Koht; *Studies of the relation of visual acuity to light energy*, by G. Durup and A. Fessard; *The spatial summation of foveal light responses*, by H. Piéron; *A study of motor skill*, by Renée Leurquin; *Some contributions to an experimental study of the course of learning*, by Dora Heller-Kowarski and Marcel François; and *An experiment on a learning curve with a 'marking' test*, by A. Chweitzer. M. Tchang and M. M. Chang write a note on *Correlations between tachistoscopic perception of varying materials*; Henri Piéron one on *The Bunsen-Roscoe law*, and S. M. Laby describes the Sixth International Congress of Industrial Psychology. The bibliographical summaries deal with 1458 different papers, and the usual chronicle of events appears.

Acta Psychologica Keijo. Vol. I, No. 1, Dec. 1930. Published by the Psychological Institute, Keijo Imperial University, Japan.

We welcome this newcomer into the field of psychological publications. The first number contains two interesting original communications, one in Japanese and German by R. Kuroda dealing with the general bodily and sensorial reactions of the new-born monkey (*Macacus cynomolgus*) and the other by the same author, in Japanese and English, reporting a study of a new case of recovery from congenital cataract.

The Primitive Mind and Modern Civilisation. By C. B. ALDRICH. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. xvii + 236. 12*s.* 6*d.* net.

The psychologist who has any time or interest to spare for the social implications of his science should read this book. Very likely he will find much in it to which he will object, for it is written from the point of view of a convinced adherent of Jung. Also there is much more about the 'primitive mind' than about modern civilization. But the book is undoubtedly a good one, full of interesting suggestions, many of which it is to be hoped that psychological field-workers will put to the test. For example, the author makes some use of the notion that *eidetic images* are common in primitive society and may account for a good many early forms of belief. It ought to be easy enough to test this; and until actual field observations are available concerning many such matters social psychology will remain mainly the speculative matter that it is to-day. However, Mr Aldrich's book is a genuine and valuable contribution to his subject, and should be widely studied.

Crime as Destiny. By J. LANGE. Translated by CHARLOTTE HALDANE. London: Geo. Allen and Unwin. 1931. Pp. 199. 6*s.* net.

Dr Lange examined thirteen cases of monozygotic twins and found that in ten of these where one was a criminal the other was a criminal as well. He also examined seventeen cases of dizygotic twins and found that in only two cases where one was a criminal the other was also criminal. He concludes that crime is destiny, and that given a certain environment it is impossible for some people to escape being criminals. He gives the detail of his study in a perfectly fair and unbiassed manner, and is in fact rather less emphatic about the predestination point than is J. B. S. Haldane, who writes a very enthusiastic foreword. Whether the evidence can bear the weight of the conclusion indicated in the title of this book or not, the book itself is a valuable study in social psychology. Mr Haldane's introduction is more clever than critical.

Love in the Machine Age. A psychological study of the transition from patriarchal society. By FLOYD DELL. London: George Routledge and Sons, Ltd. 1930. Pp. iii + 428. 12*s.* 6*d.* net.

The programme of this curiously uneven book is no mean one; it involves a discussion of love, marriage, children, art, manners, education, work and modern economics against a vaguely historical and biological background!

Its author's thesis is that psychic health in the modern world presupposes three things—full emotional adulthood, the capacity for heterosexual love, the ability for responsible work. He envisages an admittedly idealistic world where an early love choice is followed by lifelong monogamous marriage, the success of which is made probable by the emotional knowledge learned during an experimental courtship period. In this world the economic responsibilities of marriage and parenthood are shared by a wage-earning wife and a wage-earning husband, and knowledge of efficient methods of birth control may be taken for granted. This state of affairs can only be achieved by 'making people modern-minded'—*i.e.* by making them understand in sufficient detail exactly what are the differences between the psychological requirements of this machine age and those of the late patriarchal régime. It is with a discussion of these differences, therefore, that by far the largest part of the book is concerned. And though little of the gathered material is new, much of it is interesting and some of it enlightening.

Not a few of the conclusions, however, depend for their validity upon premises which from the physiological and psychological points of view are open to doubt.

It is difficult in our present state of prejudice and honest ignorance to make any statements about sexual matters which can be accepted as sound generalizations of either a positive or a negative nature. It may be admitted, for instance, that practices such as those Dell terms 'prostitution-patronage,' 'polite adultery,' 'purity,' or 'gold-digging'—all in his view distortions of adult normality resulting from the patriarchal system—are unsatisfactory compromises tending to lead to the arrest of emotional development. But is there really any convincing reason for supposing that 'petting,' the alternative to actual sex-experience which he recommends for adolescents, is psychologically or physiologically any less harmful? One finds oneself extremely dubious about this contention that the modern adolescent 'petter' and 'demi-vierge' have discovered for themselves a satisfactory way of acquiring the adequate pre-marital emotional education which perhaps all of us, now, would admit to be necessary.

Nor can one believe as whole-heartedly as the author appears to do that Psychology, the right kind of education and the Mental Hygiene Movement functioning together, will so assuredly transform and adapt socially unacceptable impulses, galvanizing neurotic fantasy-making into Art, transforming criminal tendencies into Heroism.

On the other hand, the book does systematize a great mass of discoveries and views in such a way as to make them available and comprehensible to the average kind of person whose task it is to supervise or befriend the young people of the day. But it foils its purpose by being too long, too romantic and not sufficiently scientific in spirit. Like so many of its kind too, when all is said and done, the amount of practical assistance it can offer to those it would help is but small. It was presumably written primarily for Americans, and one imagines that it will find a more temperamentally appreciative public on the other side of the Atlantic than on this.

A Contributive Society. By J. R. BELLERBY. London: Education Services. 1931. Pp. xvi + 224. *7s. 6d.* net.

This is a book which is distinctly worth reading. It falls into two parts. In the first, starting from the obvious yet little recognized principle that any system of economics is 'human in every part,' the author briefly examines the main human drives, or motives upon which most current economics is based. He discusses the growth of these motives through education, and how through education they may be modified. In this connection he gives a rapid, but clear, account of several recent educational experiments. All this is intended to lead up to the second part, in which an attempt is made to construct a picture of a society in which the main driving motive is contribution by individuals and constituent groups to the well-being of the whole. The author is throughout fully aware that "no change in the structure of the economic system should be attempted which goes *radically* beyond the powers of human character."

It may be doubted whether Mr Bellerby has succeeded in this brief book, in adequately connecting the first, realistic, and the second, utopian, part of his argument. Also the discussion, in spite of numerous illustrations, proceeds upon a somewhat general plane. But beyond doubt he has something important to say, and he has said it with great clarity and commendable brevity.

Das Genie-Problem. Von W. LANGE-EICHBAUM. München: Ernst Reinhardt. S. 128. RM. 2.80.

This is a popularly written book, more than a little dogmatic and jerky in style, often provocative in statement, but withal containing much to stimulate psychological theory, particularly on the social side. The five main sections deal with "Genius and Value" (much of this has sociological implications); "Genius and Reputation"; "Genius and Talent"; "Genius and Insanity" and "Genius and the culture of the Social Group." The whole is intended as an introduction to the author's larger work, *Genie, Irrsinn und Ruhm.* The treatment is all on the plane of general description and analysis.

I. Internationale Tagung für angewandte Psychopathologie und Psychologie. Edited by H. Hartmann, M. Pappenheim and E. Stransky. Berlin: S. Karger. 1931. S. 241. RM. 17.60.

An introduction by Dr Pappenheim is followed by a number of summaries and reports: *The significance of psychopathology for psychology,* by Rudolf Allers; *Psychopathology and religious psychology,* by Karl Beth; *Signs in man and the animals,* by Karl Buher; *Industrial psychology and psychopathology,* by W. Eliasberg; *Group psychosis and the individual,* by Th. Erismann; *Pathography and psychoanalysis,* by E. Hitschmann; *Psychopathology and heredity,* by Hans Lunenburger; *Psychopathology and criminal law,* by Hans W. Maier; *Art products of mental illness,* by W. Morgenthaler; *The meaning of complaints in psychopathology,* by Hans Prinzhorn; *Psychopathology and sociology,* by Henry E. Sigerist; *Applied psychopathology and contemporary civilisation,* by Erwin Stransky; *Vocational psychology and psychopathology,* by J. Suter; *Psychiatry and criminal law,* by S. Türkel; *Underlying metaphysics of psychological doctrines,* by R. Wälder; *The neurotic manner,* by Paul Federn; *The psychology of expression movements,* by A. Flach; *Reflexology and psychopathology,* by L. Gatzuk; *A psychiatric critique of creative work,* by K. Hildebrandt; *The significance of psychopathology for education,* by H. Kogerer; *Psychotherapy and politics,* by W. Morgenthaler; *Behaviour and personality,* by J. H. Schultz; *The physiology of hypnosis,* by F. G. Stockert.

The Psychology of Men of Genius. By E. Kretschmer. Trans. by R. B. Cattell. London: Kegan Paul, Trench, Trübner and Co. 1931. Pp. xx + 252. 15*s*.

The German edition of this work has already been noticed in this *Journal*. It is, in its English form, a welcome addition to the International Library of Psychology, etc. Kretschmer's treatment is qualitative and general and is largely concerned with reported characteristics of certain selected great men, which appear to make them fit well into his particular theory of types. The translation is very free and is all the better for that. Dr Cattell has indeed brought out fully all the lively readableness of this attractive essay, and in this way he has done his work extraordinarily well.

Hypnotic Poetry. By Edward D. Snyder. Philadelphia: University of Pennsylvania Press. 1930. Pp. x + 159. $2.00 net. (London: Humphrey Milford. 8*s*. 6*d*. net.)

The discussion begins by a distinction, with illustrations, between 'spell-weaving' poems and intellectualist poems. The author then elaborates his hypothesis, that there are poems which produce a trance or a trance-like effect and that then all have a characteristic technique which is utterly different from that of the intellectualist poem. This hypothesis is then pursued and further developed through eight exceedingly interesting chapters, and its implications for readers, listeners and critics are set forth. It is a good point of the book that illustrations are constantly given, and that a genuine sense of humour is never far distant. As a psychological contribution to literary criticism this little book has much to be said in its favour.

How to Interview. By W. Van Dyke Bingham and B. V. Moore. New York: Harper and Brothers. 1931. Pp. xiv + 320. $4.00.

If information can make a good interviewer there should be no longer any excuse for the unsatisfactory use of this method of arriving at an assessment of character. This book gives an invariably clear account of pretty well all the reasonably good work that has ever been done on the uses and abuses of the interview. In its first part it takes up the problem of how to interview for various different vocational purposes —*e.g.* in social case work, in education, in journalism, to obtain legal evidence, and so on. In the second part it gives an account of the interview as a means of finding out important facts about industrial situations. The general conclusions are favourable to the interview method. It seems a pity that words like 'interviewee' cannot be avoided.

Le Développement Mental et l'Intelligence. By HENRI PIÉRON. Paris: Félix Alcan. 1929. Pp. xii + 94. 10 fr.

Prof. Piéron here presents to a wider public four lectures which he delivered at Barcelona. Most of the facts set forth will be known to the psychologically trained reader, but, as always, Piéron's comments are full of insight and value. Briefly he attempts to study the relation of mental development in the individual and the results of a mass of intelligence testing which has been carried out in the last few years. He endeavours throughout to hold a proper balance between individual studies and the investigation of general propensities and relations by statistical method. Also while he agrees that 'intelligence' is the expression of 'the whole person,' he is convinced that its study must proceed largely by analytical methods. All the ideas in this small book are defined with delightful clarity, and the whole is an excellent example of incisive thinking and most able exposition.

Co-operation and Competition: An Experimental Study in Motivation. By J. B. MALLER. Teachers' College, Columbia University Contributions to Education, No. 384. 1929. Pp. x + 176.

The large-scale experiment reported in this monograph was very carefully devised to investigate the behaviour of children "under the motives of competition and co-operation, to measure individual differences on a scale ranging from extreme competitiveness to extreme co-operativeness, and to discover the concomitant factors that are associated with either tendency." The results are of great general interest. Among them may be noted: (*a*) that competition appears to be a more effective motive than co-operation; (*b*) both motives increase speed of work, but that of 'work for self' has a larger and a more consistent effect; (*c*) when both motives are presented in rapid alternation the work curves diverge, when each is presented independently the work curves converge. Various interesting sex differences are recorded. The whole investigation is a most useful piece of pioneer work, not only because it yielded valuable positive conclusions itself, but becauses it indicates methods and problems for further work of an equally fruitful character.

The Social and Emotional Development of the Pre-School Child. By K. M. BANHAM BRIDGES. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. x + 273. 12*s.* 6*d.* net.

The book gives (1) a social development scale, with original illustrations of each characteristic listed, and (2) an emotional development scale, also with illustrations. Instructions are given concerning the application of these scales. As a clearly worded presentation of a method of research dealing with the behaviour of young children the book will be of value to all who have to deal with the nursery school. To the psychologist the most interesting chapter is the one called "A genetic theory of the emotions." An attempt is made to fix the general periods at which differentiation of the various emotions takes place.

Home Problems of Parents. A Series of Eight Radio Talks. By FLORENCE M. TEAGARDEN. University of Pittsburgh. 1930. Pp. 90. 60 cents.

Dr Teagarden talks in a straightforward and untechnical way about difficulties in the upbringing of children which all responsible parents have to meet. Her advice is full of commonsense; her solutions practically always those which an intelligent and sympathetic parent will find out for herself. Fortunately she never minds saying, when any difficult problem of individual treatment comes along, that each case must be tackled on its merits.

The Behaviour of the Newborn Infant. By K. C. Pratt, A. R. Nelson and K. H. Seen. Ohio State University-Press. Pp. ix + 237. $3.00.

This is an intensely interesting experimental study of behaviour in the very young infant. The reactions specifically dealt with are: visual, auditory, taste, smell and temperature; reactions set up by holding the nose and arms; plantar reactions and sucking reactions. In every case the experimental technique is fully described. The authors believe that there is no evidence whatever for the gradual entry into human behaviour of a psychical factor which comes in at some period after birth. Whether they are right in this belief or not does not seriously affect the genuine value and importance of their study, which certainly marks a step forward in our knowledge of early human responses to external stimuli.

The Growth of Freedom in Education: A critical interpretation of some historical views. By W. J. McCallister. London: Constable. 1931. Pp. xiii + 589. 30*s*. net.

No brief notice can bring out adequately the excellencies of this book, which are many and of a high order. Its defects could perhaps be more justly stated, for they are relatively few, and they have mainly to do with the author's laudable desire to cover every inch of ground in a journey from Plato to Freud. This undoubtedly means a certain scrappiness in some directions, and perhaps here and there, some undue notice of less important views. Taken as a whole the book attains a very high level of sustained value. It is remarkably clearly written and shows wide learning allied to somewhat rare powers of sensible criticism. It is good history but it is much more than that, for throughout Prof. McCallister is leading up to the view of freedom as "the harmonizing of the pupil's self-assertive and submissive tendencies in the light of the highest value affirmed by him to be relevant to the life situation of the moment." Obviously any such brief formulation leaves many problems to be discussed. It is much to be hoped that Prof. McCallister will some day bring his very great ability for clear and consecutive thinking to bear upon some of these problems in a manner less tied up with historical exposition. The present book is a really good one: it may easily lead to a future book which will be better still.

Pädagogische-Psychologische Arbeiten aus dem Institut des Leipziger Lehrervereins. Edited by Felix Schotte. xviii. Band. Leipzig: Verlag der Dürr'schen Buchhandlung. 1930. S. 200. RM. 5.50.

In this volume Prof. Schotte and his collaborators publish four sets of studies. Two of these are mainly experimental and deal with varied problems of perception, memory, construction, rhythm, fatigue and the like. The other two are more concerned with social questions, both within the school and as regards the relations of school and outside groups. The whole report indicates a great amount of well-directed activity in the Leipzig Institute.

An Experimental Study of Certain Factors Affecting Transfer of Training in Arithmetic. By J. R. Overman. Baltimore: Warwick and York. 1931. Pp. vi + 235. $3.12.

This interesting monograph is distinguished from many other attempts to deal with transfer, in that it provides consistently positive evidence for 'spread' of training from one department of arithmetic to different and more or less related departments. Part of the spread appears to be automatic and part 'rational.' Transfer is said to be more marked in the more intelligent pupils. There are no significant sex differences. Largely the transfer is a function of mode of instruction. Full experimental results are given and detailed instructions to teachers appear in an appendix.

Alcohol and Behaviour. By SYDNEY SMITH. Edinburgh: Oliver and Boyd. 1930. Pp. 37. 6*d.* net.

This Henderson Trust lecture gives a most interesting and balanced account of some of the physiological and psychological effects of alcohol with special reference to the human organism. The psychological reader will find in it much material for reflection and application beyond the limits of the specific subject treated. For example the difficulties which beset all attempts to determine the exact effects of small doses of alcohol are much the same as those which confront any effort to measure the results of many other distracting or harmful stimuli of relatively small intensity. This lecture deserves wide publicity.

The Significance of the Pekin Man. By G. ELLIOT SMITH. Edinburgh: Oliver and Boyd. 1930. Pp. 20. 6*d.* net.

Prof. Elliot Smith here gives a lucid and intensely interesting account of the discovery and study of *Sinanthropus*. The lecture is illustrated by some beautifully reproduced photographs.

Don Juan and other Psychological Studies. By GONZALO R. LAFORA. London: Thornton Butterworth. 1930. Pp. 288. 7*s.* 6*d.* net.

Five essays are included in this volume. They deal with "The Psychology of Don Juan"; "Lay and Religious Miraculous Cures"; "Reflections on Inspiration in Art and Science"; "A Psychological study of Cubism and Expressionism"; and "Spiritism." They are all very excellently written and show both wide knowledge and acute observation. Dr C. S. Myers writes an appreciative foreword which is well deserved. The book will be attractive both to the psychologist and to the educated general reader.

Recent Advances in the Study of the Psycho-neuroses. By MILLAIS CULPIN and others. London: J. and A. Churchill. 1931. Pp. iii + 348. 12*s.* 6*d.* net.

Sometimes, when a work covers a very wide field, it is an advantage to have collaborators: sometimes it is not. This book falls into the latter category. One closes it feeling how much better it would have been without the last four chapters, especially Chapter x which is merely a slashing criticism of Jung's *Analytic Psychology.*

The first eight chapters, by Dr Culpin, are very good. In them he gives a critical account of recent work, much of it his own. It has for the most part been published elsewhere; but it is very useful to have it collected, summarized and worked up into a coherent whole.

The chapters dealing with occupational neuroses and the psycho-neuroses of industry should be read by all general practitioners; while those who are dissatisfied with the present, rather chaotic, classifications will welcome Dr Culpin's remarks and suggestions.

Die Prognostik der Endogenen Psychosen. Von FRIEDRICH MANZ. Leipzig: Georg Thieme. 1930. S. 121. RM. 7.50.

This study of dementia praecox and manic depressive insanity is written from the point of view of the present Marburg school and is preceded by a foreword by Dr E. Kretschmer. It is a condensed and workmanlike survey of the clinical facts, with, of course, a certain admixture of theory, and is obviously the outcome of much careful original observation. Whether he is willing to accept the particular psychology of types which this monograph represents or not, a student of psychiatry will find much to interest and aid him in this essay, for the whole treatment is refreshingly definite, clear-cut and brief.

Manuel de Philosophie. Tome I: *Introduction Générale: Psychologie.* Par A. CUVILLIER. Paris: Armand Colin. 1931. Pp. xxxix + 722. 38 fr.

In this vast text-book most of the general and pathological, and not a few of the experimental, problems of psychology are dealt with. The treatment suffers from the fact that the author seems to want to set forth most of everything that he knows. The result is a great display of learning—most of it accurate enough—but little evidence of critical capacity, and it is hard to see what the student will make of it all.

The Recovery of Myself. By MARIAN KING. Newhaven: Yale University Press. 1931. Pp. 148. (London: Humphrey Milford. 9*s*. net.)

This is a fragment of autobiography. It gives the experiences of a patient at a mental hospital, whither she went for drug treatment. On the whole she had a good time, came out cured, and writes appreciatively of her treatment. The story is often of psychological value and it is well written, but the book is irritatingly printed in large type.

The Growth of the Mind. By ROBERT ARMSTRONG-JONES. Edinburgh: Oliver and Boyd. 1929. Pp. 29. 6*d*. net.

This is a Henderson Trust lecture delivered at Edinburgh University. In it Sir Robert Armstrong-Jones rapidly traces some main points in the development of the nervous system, and makes a number of remarks about mental process. Nothing new is said.

O Exame Alpha nos universitarios de Pernambuco. Por A. B. DE OLIVIERA FIRMO. Pernambuco. 1930. Pp. 65.

The author gives a brief account of the development and applications of mental tests. He gives the American Army Alpha Tests adapted for use in South America and some details of their application to university students and others.

Estudo psychotechnico de alguns tests de aptidão. Por U. PERNAMBUCANO and A. P. BARRETTO. Recife. 1927. Pp. 43.

In this study the results are presented of the application to South American school children of five selected and adapted mental tests.

Actas de la Primera Conferencia Latino-Americana de Neurologia, Psiquiatria y Medicina Legal. Buenos Aires: Imprenta de la Universidad. 1929. Vol. I, pp. 707; Vol. II, pp. 965.

The bulk of the papers in this report of proceedings is concerned with histological and anatomical problems. There are, however, a considerable number of clinical studies of functional disorders, and a few papers of very great interest from the social psychological point of view. The whole bears witness to marked activity of psychiatrical and psychological study in Latin America.

Psicologia dell' Età Evolutiva. By ENZO BONAVENTURA. Lanciano: Giuseppe Carabba. 1930. Pp. 251.

This volume gives a general survey of recent work in genetic psychology in relation to young children and adolescents. An introductory section discusses the main general heads of treatment; a first part deals with specific studies in the development of perception, thought, language, memory, imagination and affect; a second part treats in some detail the modern use of intelligence test methods. The whole makes a concise but very well-informed volume.

Curiosities of Heredity. "The Hapsburg Lip." By J. N. UNFOLD PITT. London: John Bale Sons and Daniellsson, Ltd. 1931. Pp. 24. 2*s.* net.

This is not a serious work, but merely a few scattered notes upon 'the Hapsburg lip' and other bodily characteristics, mixed up with some stories and a number of expressions of opinion about heredity.

Mental Measurement of Pre-School Children. By RACHEL STOKSMAN. New York: World Book Company. 1931. Pp. xii + 368. $2.20.

Part I gives an account of earlier work on the topic of the book and discusses criteria according to which tests for children up to six years of age should be selected. Part II describes the work and its results which led to the establishment and standardization of the Merill-Palmer scale. Part III gives complete instructions for administering the tests. Part IV consists of a number of illustrative case studies. The book is clearly written, and should be of considerable value in the nursery school.

Die Jugendlichen und ihre Erzieher. Von M. DÖRING, ALOIS LORBER, HERMANN POST, HELENE SCHEUCHER, und C. TUMLING. Langensalza: Hermann Beyer und Söhne. 1930. S. 49. RM. 1.30.

Here are six brief case studies of young adolescents which throw some light upon difficulties, both normal and exaggerated, incidental to this period of school life.

Die Psychologie des Grundschulkindes in ihrer Beziehung zur kindlichen Gesamtentwicklung. Von OSWALD KROH. Langensalza: Hermann Beyer und Söhne. 1930. S. 352. RM. 6.60.

Prof. Kroh's text-book has had a considerable popular success and here reaches its eighth edition. It deals generally with pedagogical psychology in its more normal aspects. It is clearly and concisely written, though it shows very little acquaintance with work described in any other language than German. The book may be safely recommended to any English student who is interested in a general way in German educational theories.

Die Psyche der weiblichen Jugend. Von ELSE CRONER. Langensalza: Hermann Beyer und Söhne. 1930. S. 92. RM. 2.25.

This essay is written almost entirely in general terms, but it is of considerable social and psychological interest. It discusses first the general field of the psychology of the adolescent woman, dealing with the main types of character developed during this period, with questions of individual treatment especially in relation to religion, friendship, love and art, and with the part of young women in general social relationships. More briefly it considers the special character of the psychology of young women at the technical school and in the wage-earning class. Finally a short section deals with psychopathological problems. The whole discussion is set into close relation to current social movements, especially in Germany.

Das Generationsproblem in der deutschen Dichtung der Gegenwart. Von HEDWIG KOCH. Langensalza: Hermann Beyer und Söhne. 1930. S. 116. RM. 3.00.

The more or less popular literature of all countries in most ages undoubtedly reflects current opinion with regard to the relations between younger and older generations, though often the opinions are those only of sections of a community. In this way, as in others, such literature provides an attractive subject for psychological study. Dr Koch's monograph will, therefore, be attractive to any social psychologist who is interested in current problems, and it may well stimulate some other writers to carry out a similar study in different communities.

Experimentelle Untersuchungen über die Entwicklung der Gedächtnis- und Denkformen der Knaben und Mädchen zwischen 7–19 Jahren. Von L. NAGY. Jena: Gustav Fischer. 1930.

This is an off-print from the Proceedings of the Eleventh Congress of Experimental Psychology held in Vienna in 1929.

Psychoanalyse und Pädagogik. Von ERNST SCHNEIDER. Langensalza: Hermann Beyer und Söhne. 1930. S. 72. RM. 2.30.

Prof. Schneider writes mainly in general terms, but with some illustrative material, and aims at expounding for the working teacher the significance for his field of practice of psycho-analytical study and theory. As far as it goes his essay is clear and straightforward, and should serve as a useful introduction to a more thorough study. A list of relevant works which will help this more complete study is provided.

Eros und Sexus. Von GERHARD PFAHLER. Langensalza: Hermann Beyer und Söhne. 1930. S. 42. RM. 1.25.

Here is an essay of the social psychological kind written for the rising generation. It contains many interesting remarks about the place of love and sex in modern movements in art, sport and education.

Marcel Proust, sa Révélation Psychologique. By ARNAUD DANDIEU. Paris: Firmin-Didot et Cie. 1930. (London: Humphrey Milford.) Pp. 208. 3*s.* 6*d.* net.

This is a very interesting study, from the point of view of normal and abnormal psychology, of the writings of Marcel Proust. It is naturally concerned largely with Proust's use of metaphor, but discusses also some of the main influences which moulded his work. The final section, which deals with "The progressive loss of a sense of reality in Proust," is particularly well worked out.

The Nature of Living Matter. By L. HOGBEN. London: Kegan Paul. 1930. Pp. ix + 316. 15*s.* net.

This book is very brilliantly written, but it is a pity that the different essays of which it is composed have not been partially revised in order to avoid the frequent and rather tiresome repetition of the ideas, often in nearly the same words. The book deals in only a few places with the nature of living matter—sadly too few in view of the force and clarity of the exposition in these places. It is devoted for the most part to criticisms of the "arrogant assumption that...introspective philosophy is more fundamental than science"; it contains many illustrations from history to demonstrate how often the philosophers have been wrong and how often the scientists have been right; and it contains evidence to show that the only advances in truth and reality are to be made through the medium of ethically neutral empiricism, that science is more fundamental than anything else, and that biology, which should always remain ethically neutral, is the most important of the sciences.

Reason and Nature. By MORRIS R. COHEN. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. xxiv + 470. 21*s.* net.

This is a book in the grand manner. It covers a huge field, and discusses, usually in general terms, but often with some particularity, a vast number of specific problems. It has three 'books.' The first book discusses modern forms of reaction against reason, and modern substitutes therefor. It also considers the "metaphysics of reason and scientific method." The second book is concerned with reason in natural science, that is to say in mathematics, physics, biology and psychology. The third book, which many readers will find the most interesting, is concerned with reason in social science.

The volume is uniformly readable, with lots of epigrams and bright sayings scattered about. Perhaps it is fair to say, however, that the fundamental terms and notions used are not very decisively defined, and that if they had been the discussions might have been considerably condensed.

A Study in Aesthetics. By LOUIS ARNAUD REID. London: George Allen and Unwin. 1931. Pp. 415. 15*s*. net.

Although this book is in the main of philosophic import, it contains much that will interest the psychological reader. Very broadly aesthetic experience is defined as the enjoyment of valuable meaning for its own sake. This is made much less vague than might appear by a copious use of illustration and criticism. Dr Reid has much sympathy for the view of beauty as some form of 'expression,' and here his utmost efforts leave the significance of this troublesome term hard to grasp. However, this book is a sincere and valuable piece of work in a difficult field.

Novius Organum. By J. C. MCKERROW. London: Longmans, Green and Co. 1931. Pp. viii + 277. 9*s*. net.

The sub-title of this book is *Essays in a New Metaphysic*; but let no psychologist be frightened away by this. Mr McKerrow has collected a number of short papers on a variety of topics, but the burden of them all is that everything that happens, and everything that is, happens as it does and is what it is by virtue of accidental habit which continues through mere repetition. This is naturally a big burden to lay upon accident, habit and repetition, but the author writes always in so lively a way and disagrees with numerous eminent people in so gay and decisive a manner, that, whether he convinces anybody or not, he will stimulate thought in all of his readers. Some of the essays, *e.g. Extruding the subject*; *Affection and conation*, *The biogenic psychoses* and *The social psychoses* are directly psychological in their challenge. They all raise long-standing questions, but even now most psychologists would be all the better if they had to face up to Mr McKerrow's difficulties and find answers that still left a place for their science.

Fragen der Ethik. Von MORITZ SCHLICK. Wien: Julius Springer. 1930. S. 152. RM. 9.60.

Prof. Schlick has acquired a reputation in this country in many quarters for his acute and clear thinking about ethical problems. In this book he expounds his views in a particularly clear and straightforward manner. The psychologist who has a bent for ethical problems will find much to interest him, especially in the sections dealing with pleasure and with instinctive trends.

The Living Mind. Essays on the Significance of Consciousness. By WARNER FITE. London: Williams and Norgate. 1931. Pp. ix + 317.

The essays range over a large number of topics from *Eucken's Philosophy of Life* to studies in the psychology of advertising and in birth control. In most of them psychology plays a sort of hide-and-seek game with ethics and philosophy. They are all very lively and readable, all for the psychical process as something possessing independent status, none of them very thorough, or particularly original.

Behind the Electron. By W. H. LEWIN. London: C. W. Daniel Company. 1931. Pp. 101. 3*s*. 6*d*. net.

This contains a long chapter on human personality which is mainly a blend of the popular and the speculative.

Infinity and Ego. By J. C. ISBYAM. London: C. W. Daniel Company. 1930. Pp. 25. 1*s.* net.

Über das Hierarchische Prinzip in der Natur und seine Beziehungen zum Mechanismus-Vitalismus-Problem. Von EWALD OLDEKOP. Reval: F. Wassermann. 1930. S. 64.

Hauptsätze der Metaphysik. Von BRANISLAV PETRONIEVIES. Heidelberg: Carl Winter. 1930. S. 82. RM. 5.

The Central Problem of David Hume's Philosophy. By C. V. SALMON. Halle: Max Niemeyer. 1929. Pp. vii + 449. RM. 7.

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THE PERCEPTION OF TACHISTOSCOPICALLY EXPOSED SYMBOLS[1].

BY F. AVELING.

(*From the Psychological Laboratory of University of London, King's College.*)

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II. *Material and technique of research* (p. 194).
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V. *Insightful apprehension and associative contamination* (pp. 196–198).
VI. *Conclusions drawn from nature of errors* (p. 198).
VII. *Degree of optimal conation* (p. 198).
VIII. *Bearing of data on the problem of form perception* (pp. 198–199).

I. ORIGIN OF INVESTIGATION.

IN a previous research on "The influence of affection upon perception[2]," carried out by Dr Saidullah in the laboratory of King's College, several interesting phenomena came to light.

1. Groups of block letters exhibited once tachistoscopically (30–100 sig.) were invariably perceived as 'groups of letters.'

2. Frequently individual letters were correctly perceived, but were not located, or were wrongly located in relation to each other.

3. Frequently also spatial relations were perceived, but not the determinate letters spatially so related.

4. Single letters in the groups were frequently 'deformed'; *i.e.* integral parts were missed out, additional parts were reported, or the letters were broken up (VV seen for W) or inverted, etc. One or several of such 'deformations' might occur in respect of the same letter.

These phenomena suggested the drawing of a distinction between the groups as 'wholes' and the letters, together with their inter-relations within the groups, as 'elements', as well as a similar distinction between the letters themselves taken as 'wholes' and the elementary parts together with the relations which constituted them as letters. They further suggested the distinction between noegenesis and reproduction as processes jointly concerned in perception.

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[1] Communicated to the Ninth International Congress of Psychology, 1929.

[2] D.Sc. thesis in the Library of the University of London.

The phenomena in question were not relevant to the main line of Dr Saidullah's work; but their indications, briefly communicated at the time to the British Psychological Society, seemed to warrant further investigation. A second research was accordingly planned and carried out by Mr A. G. Caws and the present writer upon the manner in which groups of letters ('wholes'), letters ('elements') entering into the groups, and relations constituting the 'elements' as 'grouped,' were perceived.

II. Material and technique of research.

For this purpose groups of six block letters (found by preliminary experiment to be most suitable for the investigation), were exposed by means of a Michotte Rotary Tachistoscope for 50 sig.; the distance of projection being such that optimal visibility was secured for all the letters. Each group was exposed again and again until the subject was confident that he had perceived it correctly; but after each exposition what had then been seen was noted by him, and his introspection was recorded. In all twenty subjects participated in the research.

The investigation embraced a number of series of letter-groups, of which only four are considered in the present paper. In the first of these the groups consisted of consonants chosen without reference to the fact that some might equally well be read backwards or forwards (as MBXFTR). In the second series only letters which might be read either way were included in the groups (as XMVHTW). The third series consisted of groups similar to those of the second, with the exception that one forward-looking letter, variable in position in the group, was included and printed mirrorwise (as ꟻMXTHW, WXꟻHVT, VMTXHꟻ). Lastly, in a fourth series were included the vowels A, I, O, U, Y, which might be read forwards or backwards. The groups in this series formed nonsense words rather than mere groups of letters; and in them a letter was reversed as in the third series.

The instruction given was in every case the same: "You will be shown a number of letters. Perceive as many as you can, record them, and then write your introspection."

III. Types of perceiving.

In every case, and in all the series now reported, the subjects 'perceived' a 'whole' group of symbols, some of which were not apprehended as determinate letters nor, even when determinate, in the spatial relations in which they had been presented. At an early stage of experimentation it appeared that subjects tended to fall into one of two groups:

(*a*) Those who apprehended a 'whole' of definite dimension of which only two or three initial letters were determinately perceived, the others being mere 'somethings.' In the case of such subjects the building up of the 'whole' as a group of determinate letters took place by way of serial addition to the letters first perceived during the subsequent expositions. An interesting point here is the fact that the 'whole' as first perceived is limited at either extreme, in spite of the fact that the final limit is absolutely indeterminate.

(*b*) Those (more numerous) who apprehended a 'whole' in which determinate initial and final letters were clearly perceived, the rest being again merely undifferentiated 'somethings' partially constitutive of the 'whole.' These subjects built up the 'whole' as a group of determinate letters by gradually adding here or there determinate intermediaries spatially related to each other and to the initial and final letters.

It is not suggested that these 'types' indicate any radical individual differences, but the variations in procedure seem to warrant notice.

IV. ERRORS IN PERCEPTION.

Several important kinds of error in perception were observed in all the series, of which only three salient examples will be reported in the present communication. In the earlier serial expositions of the groups errors were of frequent occurrence in the case of all the subjects, and were recognized in later expositions definitely as having been erroneous.

(*a*) Letters (or position of letters relative to each other) were reported as 'missing'; *i.e.* subjects knew that they had been exposed, but could say nothing definite about them (what or where they were). Strictly speaking, this does not involve error, since nothing is accepted which is not true in their regard; but the observation is an interesting one, because it indicates that the letters may be apprehended without their spatial relations, and that relations may be apprehended without any determinate fundaments.

(*b*) Letters were apprehended correctly, but in incorrect order; as, *e.g.* TJM for TMJ. Here there is no error in respect of the fundaments, but the relations are definitely erroneous.

(*c*) Letters themselves were incorrectly apprehended, but the relations between them and contiguous letters (*i.e.* the order) were exact.

An example is X-P-L seen for X-R-L. Deformation of the individual letters principally consisted:

(i) in the omission of parts of the letters in question (as P seen for R);

(ii) in the addition of parts to the letters (as R seen for P). Some letters in particular seem to lend themselves to this kind of deformation;

(iii) in inversion of the letters (as M seen for W);

(iv) in separation of parts of the letters (as VV seen for W);

(v) in agglutination of letters (as W seen for VV).

From a consideration of errors such as those exemplified under *c* (1), (2) and (3), it would appear not only that correct spatial relations may be perceived between erroneously apprehended letters, but also that the individual symbols entering into the groups are themselves 'wholes.' Thus the letter R, for example, is constituted as a 'whole' of the elements I ɔ ⟍; and these elements, together with the relations between them, behave in respect of error in precisely the same way as do the letters related in the groups. As in the case of the groups, it is clear here also that omissions of such fundaments or additions to them, as well as alterations of relation between them, may be causes of error in perception. The two cases are parallel.

V. INSIGHTFUL APPREHENSION AND ASSOCIATIVE CONTAMINATION.

The precise incidence of such errors might be attributed *a priori* to the actual process of perception, to retention or to recall. So far as it may be reliable, however, introspection points to actual perception as the source of the error. Subjects clearly distinguish between perceiving and writing down what they perceived with certainty on the one hand, and perceiving and then forgetting on the other. The case of 'missing' letters seems certainly to be due to imperfect apprehension, *i.e.* lack of intensity or determinateness of the items cognized. In so far as known at all, even as 'missing,' such items are insightfully cognized. A similar observation holds good with regard to the insightfully apprehended letters (or relations) when the relations (or letters) are not determinately cognized. In like manner the omission of elementary parts, or relations between the parts, of individual letters might be explained. But the distortions by addition, inversion, separation and agglutination are in a quite different case. The letters so distorted cannot have been insightfully apprehended. The error arises in perception, not in apprehension; and the deformation must have been due to some process superadded to this latter. This is tantamount to saying that the superadded elements or relations were recalled from past experience by way of association. Indeed, even the omissions *may* have been influenced by associative processes also.

The distinction is here drawn between the insightful apprehension of directly presented items and relations (Spearman's *noegenesis*) and the contamination of these by factors due to retentivity. Errors in perception are due solely to the latter, never to the former.

A further type of error of extremely interesting character, emphasizing the foregoing distinction, occurred in series III and IV, in which one letter was printed mirrorwise.

In these series, again, the groups were, with one exception (below), invariable perceived as 'wholes'; but the reversed (mirrorwise) letter conditioned heretofore unreported phenomena in the perception, according to its position within the group; and the introspective accounts of the process were borne out by the objective records of the letters as reported. The phenomena in question are as follows:

(1) In the initial position a reversed letter is rarely unperceived, and in this case there is a marked tendency for subjects to perceive the 'whole' as a group in which *all* the letters are reversed. (When, for instance, groups of these series were first unexpectedly exhibited to the relator, he found himself afterwards actually *writing down* the letters from right to left.) Moreover, the reversed letter in this position increases the number of expositions necessary to produce confidence in correct perception.

(2) A medially reversed letter, while causing inhibition, causes less than an initial or a final one.

(3) When the reversed letter occupies the final position it easily escapes detection; most frequently it is incorrectly perceived (as N for И, Q for Ƨ); and, even when not recognized as such, strongly inhibits perception of the other letters of the group.

(4) With regard to these inhibitions, it is noteworthy that they occur during the first few serial expositions only. Inhibition of the neighbouring letters gives place to facilitation (and increased clearness) of them once the reversed letter has clearly been apprehended.

(5) Not all reversed letters operate to the same degree in the respects noted. Reversed F and K, for instance, produce more striking results than N or S.

(6) When vowels were introduced (series IV) certainty of correctness of perception was most quickly reached; though subjects were not aware of the fact, fewer exposures were necessary. Moreover, some subjects showed a tendency to break up the 'wholes' (groups) into two (as HAW

TOX). Nevertheless, the introduction of reversed letters here increased the difficulty of perception also. Forward reading occurred up to a reversed letter; then the tendency to read backwards was manifest. To this were attributed many errors of order (relation) in the letters reported as having been perceived.

VI. CONCLUSIONS DRAWN FROM NATURE OF ERRORS.

Three principal conclusions are drawn from the nature of the foregoing errors:

(1) That the inhibitions are due to the drainage of mental energy towards perception of the reversed letters.

(2) That the errors are to be attributed to confluent associations induced by them.

(3) That their influence can be exerted marginally or subconsciously in both respects.

VII. DEGREE OF OPTIMAL CONATION.

In Saidullah's research it was definitely proved that the amount of perception (number of letters perceived correctly) was greater during the attitude of activity than during passivity, up to the limit of the span of apprehension; but that the difference in amount in these two attitudinal conditions was less than the difference measured as between the attitude of passivity with and without accompanying (irrelevant) hedonic stimuli. Pleasurable and painful accompaniments, in the conditions of his experiments, enhance perception even more than does direct conation. In the research presently reported no attempt was made to secure the two attitudes for purposes of comparison. It was nevertheless noticed that increased trying to perceive correctly during the serial expositions not infrequently appeared to result in worse perception, both as to amount and to error. Since one of the chief difficulties in the former research was to secure the attitude of passivity and maintain it, the conclusion seems warranted that in these experimental conditions the normal mental state is one of maximal or optimal conation, and that to seek to increase it is to divert energy from the act of perception itself. This interpretation of the facts agrees with observations of Wild (Mental Tests), Cox (Manual Dexterity), Walters (Retentivity) and others.

VIII. BEARING OF DATA ON THE PROBLEM OF FORM PERCEPTION.

The experimental data rehearsed in this communication point towards a solution of the problem of the immediacy of form perception. What was invariably reported was the perception of a 'whole' group of letters, in which, until perception was complete, some letters and some relations were clearly perceived. (So also for individual letters.) Further letters were apprehended as 'somewhere,' and relations were apprehended in the absence of determinate letters. The conclusion seems to be that both elements and relations can function subconsciously in constituting a 'whole'; and that the controversy as to the single- or dual-moment nature of the process cannot be settled by introspection. It is generally agreed that associative processes are involved in perception; and accordingly it is to apprehension that appeal should be made in order to determine the matter. In the present state of our knowledge there is little, if any, experimental evidence to guide us here; and recourse must largely be had to speculation. Clearly, complex percepts may be cognized without cognition of relation between them; and the awareness of these relations may later emerge, and the related percepts *be* a 'whole.' But this is not so clear in the case of the simplest apprehends, as a coloured surface. These invariably seem 'given' as 'wholes', elements already in relation. Since, however, we can trace the subsequent genesis of relations between percepts already cognized as fundaments, it would not seem unreasonable to infer theoretically that the perception of *all* 'wholes' is a dual-moment process, in which the relation is in fact (though not introspectively), superimposed upon the elements.

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SOME EXPERIMENTS ON THE ABSTRACTION OF FORM AND COLOUR.

PART I. TACHISTOSCOPIC EXPERIMENTS.

By O. A. OESER.

(*From the Cambridge Psychological Laboratory.*)

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V. *Results* (pp. 206–212).
VI. *General conclusions. Form and colour reactors as psychological types* (pp. 212–215).

I. Preliminary observations.

It is an everyday observation that some people 'have an eye' for forms, others are more attracted by colours. The first remember the shapes of things they have seen, the second their colours. A personal experience may serve as illustration. In September 1929, the author was returning from the British Association meetings in South Africa. On board were an American professor and his wife. On stormy days a few invincibles used to gather round them and play jig-saw puzzles. We noticed that when Mr and Mrs W. were together they could solve the most complicated jig-saws at record speed, but when either was alone, he or she was not faster than the rest of us. The reason, as they themselves told us, was that he 'saw' only the shapes of the pieces, she only the colours. Neither had any eye defects, such as colour blindness or faulty refraction. Thus, whenever a situation arose in which a certain measure of concentration and abstraction were called for, each abstracted only one content from the perceptual situation.

The first to have noticed this in experimental work seems to have been Külpe (9). He performed some tachistoscopic experiments on abstraction, in which he found that one of his subjects took more notice of colours, another of forms, whilst the third took up an intermediate attitude. With such a small material it is not possible to say whether we are dealing with different psychological *types*. On the basis of his quantitative results, and his general observations, Külpe comes to the conclusion that the form abstractor is a 'formal' type, the colour abstractor a 'material' type. He asks, whether the differences found are due to differences in the 'anticipatory factors,' or differences in the visual sensations themselves. Further work went to show that it was actually a case of different *sensations* and not of different *attitudes*. A. Descoeudres (2) examined a large number of children and found that a certain number of her subjects always judged according to the colour, others according to the form. As the age of the children increased, the percentage of form abstractors increased. D. Katz (7), G. Révész (13, 14) and H. Volkelt (18) all pursued similar researches, and Katz and Volkelt found, in agreement with Descoeudres, that more children are colour abstractors than form abstractors. M. v. Kuenburg (10), on the other hand, came to the opposite conclusion. M. Elljasch (3), however, showed that v. Kuenburg's material and instructions were faulty, and that Katz's conclusions were valid even under widely varying experimental conditions. Only when the selection of forms is made particularly easy, and the instruction given to the subjects is of a different kind altogether, can a colour abstractor be led to prefer forms.

None of the above investigators studied the problem from the point of view of psychological types. They were only interested in general aspects. But in 1927 R. Scholl (17) published two papers on the abstraction of form and colour in which he showed that definite characterological and somatic characteristics were correlated with colour or form dominance in his experiments. Since then several other papers have been published by Lutz (11), Poppinga (12), and Ritter (15). The first named, like Scholl, made use of Kretschmer's (8) typology, the last two of Jaensch's (5, 6). All these observers did tachistoscopic experiments (besides others), and it is on these that they lay most emphasis. But the results are by no means uniform, and it seems clear that some of the discrepancies are due to faulty technique, or to factors that vary with the type of material presented. It seemed worth while, therefore, before proceeding with experiments whose aim was the production of a number of pure types, to perform a more critical analysis of the presented material.

II. APPARATUS.

The experiments were performed with Schumann's form of pendulum tachistoscope. It consisted of a 12-gauge aluminium disc, containing a movable sector, which could be opened from 0–90°. At one point of the circumference a short arm with sliding weights was fixed, so that the speed could be regulated if necessary. A catch at a fixed height enabled the disc to be dropped always from the same height. The apparatus was set up 8 ft. from a blank white wall. Behind it was a projection lantern. A small circular spot of light was thrown on the centre of the field projected by the lantern and served as a fixation mark.

III. MATERIAL.

In former investigations no notice was taken of the 'relative intensity' of the forms and colours used. Scholl, for instance, used a series of deformed squares, which were not easy to distinguish from one another, together with a number of colours, some of which were intenser than others. Descœudres, again, in her investigations with children, found that when simple geometrical forms were used, the percentage of colour-dominant subjects was very much higher than when definite forms like 'basket' or 'lamp' were used[1]. Again, Scholl had drawn the outlines of his forms with black ink, so that it is conceivable that the form aspect was emphasized unduly.

Preliminary experiments were therefore performed to get a rough scale of increasingly intense forms and colours. Slides were projected on which were a number of forms of the same colour (see p. 203). The forms that were seen most frequently are shown in Figs. 3 and 4, those seen least frequently in Figs. 1 and 2. Similarly slides were made in which a certain shape (*e.g.* a square) appeared in different colours. Here red had the greatest frequency, then came green, light blue, light yellow.

The material for the main experiment was now prepared as follows (cf. Fig. 5): along the circumference of a circle 3 cm. in diameter, eight geometrical shapes were cut out of opaque black paper. Stencils were used, so that the shapes were the same size in all the plates. Pieces of coloured gelatine paper, such as is used in lantern-slide work, were pasted over the shapes, and the whole enclosed between two quarter-plate (lantern-slide) glasses. Half the slides had eight figures, half had an additional one in the centre. In this way two possible sources of error in Scholl's work were avoided. He had arranged his figures in the form of a square and had found that the corners were preferred by most subjects. Also there is inevitably a little more crowding towards the corners. If the shapes are arranged in a circle, they can be equally spaced. Furthermore,

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[1] Thus, for children from 3 to 6, the percentage of form and colour seers are: in the first case 69:31; in the second case (meaningful forms) 38·5:61·5. For children from 15 to 18 the change is even more striking: from 36·5: 63·5 to 18·5 : 81·5.

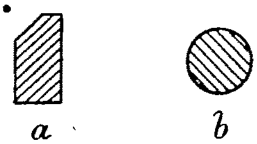

Fig.1

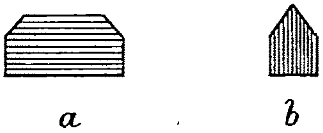

Fig.2

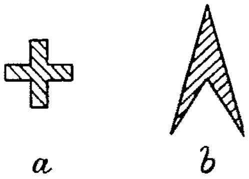

Fig. 3

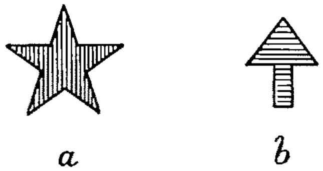

Fig.4

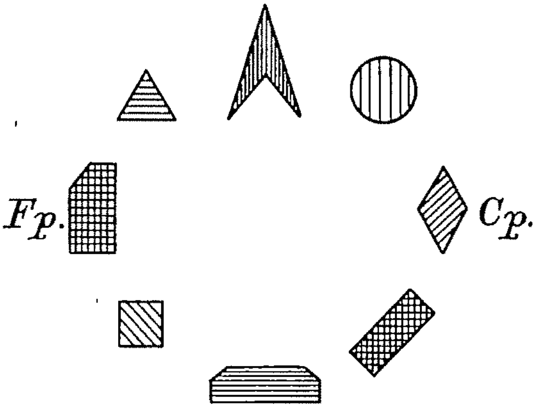

Fig. 5

KEY

▨ *Yellow*
▥ *Green*
▦ *Mauve*
▧ *Blue*
▤ *Red*
▥ *Orange*

it was found that only one or two subjects showed a preference for the top right-hand corner, which Bartlett had previously noticed in his work (1).

The main figures (Figs. 1–4) were cut out on separate slides. Four series were made[1]:

Series I, a 'weak' form in a 'weak' colour (Fig. 1).

Series II, a 'weak' form in a 'strong' colour (Fig. 2).

Series III, a 'strong' form in a 'weak' colour (Fig. 3).

Series IV, a 'strong' form in a 'strong' colour (Fig. 4).

Each series consisted of four slides, a total of sixteen exposures. The main figure appeared in each slide in the same size and orientation as in Figs. 1–4. In six cases it was also of the same colour. In the other ten cases, form and colour were split. The place in which the main figure appeared we shall call the *form pole* (Fp); the place at which the colour appeared, the *colour pole* (Cp). In each series two main figures (MF) were used.

IV. METHOD.

(1) The room was in darkness. Apparatus and observer were behind a screen. The subject was seated 6 ft. from the wall, and was given sufficient time to adapt his eyes.

(2) Each subject was tested for colour blindness, (*a*) by means of Holmgren's wool fibres, (*b*) by a slide containing the various colours used, which he had to name. Only one case of red-green blindness was found and the subject discarded.

(3) The time of exposure was 100σ. In some cases, with specially good and accurate observers, 50σ was used. Katz had used 500σ, Scholl 200σ and Lutz 4σ. 500σ is undoubtedly too long, since every subject was able to find both the form and the colour in that time. On the other hand, 4σ seemed much too short, since far too many mistakes were made with such short exposures. It is curious that neither Poppinga nor Lutz used slides on which Fp and Cp coincide, and that neither mentions failures. Poppinga states that in *every case* (at 4σ!) there was a reaction either to the Fp or to the Cp. In conversation with me he admitted that occasionally a failure had occurred. He states (p. 2) "in allen Fällen erfolgte spontan die Entscheidung für einen der beiden Pole...ausser bei einem 'trockenen' Engländer, wegen Mangel an Rapportfähigkeit." After the exposure the subject was asked on what part of the field his eye was

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[1] After the experiments had been completed I found that Lutz (11) used a similar method (opaque paper as background); but he still kept the square arrangement. Poppinga merely repeated Lutz's work.

resting. If, for example, the Fp was at the bottom, and the subject said, "somewhere down on the left," this was taken to be a proof that his eye had been attracted by the Fp. Scholl's method was similar. Now for short exposures, when there had been a failure to find either Fp or Cp, few of my subjects could state definitely where their eye had 'slipped' to; and if they could or did, they were as often far away from either pole as near it. This may be due to the fact that they were all 'trockene Engländer,' and most of them of a type that is habitually cautious in its statements. Be that as it may, after a number of trials 50–100σ was fixed upon as the shortest useful exposure.

(4) After the necessary preliminaries, the subject was shown the first MF for about 10 sec. and asked to look at it carefully, so as to be able to recognize it. He was then told, "On the word 'ready'! look at the fixation spot. After 2 sec. I shall say 'now'!, when a field of coloured figures will flash out. The MF will appear somewhere in the same size and position, but not necessarily in the same colour." Immediately after the exposure the light in the room was switched on, to prevent identification by means of the after image. The subject stated where he had seen the MF, and was then asked to describe what else he could remember. A slight further advantage of the circular arrangement was that the subject could make a definite statement about the position of an object he had seen, *e.g.* 'at 5.30 o'clock.' To make sure that he had understood, the subject was first given a few practice slides.

(5) Previous investigators only took the reaction to Fp or Cp into account. It seemed worth while to introduce a more systematic method of marking the results. Scholl states (p. 246) that the form-dominant subjects were mostly able to recognize the colour of the Fp as well, while the colour-dominant subjects were, on the whole, unable to describe the form of the Cp correctly. We also found that the colour-dominant subjects were not as accurate as the others, but, in most cases, they too, were able to describe the form of the Cp correctly. Moreover many subjects were able to find *both* Cp and Fp occasionally, particularly in series III. The marking adopted was therefore: *F*C, *C*F, F, C; that is, a reaction to the Fp was only called a form reaction, F, if the subject was certain that the Fp also had the colour of the MF, when in reality it had appeared in a different colour. Similarly for the Cp. *F*C (or *C*F) mean that the Fp (or Cp) had been found, and that its colour (form) had also been perceived. The further statements that were made about the objects in the field, we also marked FC, if both form and colour had been correctly seen, F and C if statements about shapes only, or colours only, were made.

Subjects. There were nineteen women and thirteen men. Six women and three men were students at the School of Arts and Crafts, Cambridge. The rest were university students of all faculties except theology. I have to thank Mr Hookam, of the School of Arts and Crafts, for sending his pupils to me, and allowing me to do a part of the investigation (Rorschach Tests) in the school.

Table I. *Distribution (all subjects).*

% form reactions	No.	% of subjects
100–81	1	3·1
80–61	10	31·3
60–41	12	37·5
40–21	7	21·9
20–0	2	6·3

Table II. *All subjects.*

	F 71–100	FC 51–70	CF 31–50	C 0–30
No. of subjects	7	10	11	4
Average % of F reactions	78·4	59·6	40·7	19·5
Divergence	(12·5)	(7·1)	(9·3)	(8·4)
Average no. of answers	26·6	32·1	31·3	37·0

Maximum F % = 90·9; Minimum F % = 11·1.

V. RESULTS.

Table I gives the distribution of the subjects between 0 and 100 per cent. form abstraction.

From Table II onwards the subjects are divided into four groups, F, FC, CF, C. In group F are those subjects who gave from 71–100 per cent. form answers, that is, they found the form pole but not the colour pole in most instances. We shall call them 'form dominant,' or simply the 'form type.' FC is the 'form-colour type,' with 51–70 per cent. form answers; CF the 'colour-form type,' with 31–50 per cent.; C the 'colour type' or colour dominant, with 0–30 per cent. Subjects under F and C are relatively pure types, as will be shown below; subjects under FC and CF are 'mixed types,' whose behaviour varies under different conditions. For some purposes groups F and FC, C and CF are taken together. In these cases we shall call F and C the 'strict types,' F + FC and C + CF the 'relative types' of form or colour dominance.

Table II gives the distribution over these four groups in the first row. The second row gives the average number of form answers (expressed as a percentage of the total number of answers in each of the series). The figures in brackets give the maximum divergence in each group. The last row is particularly interesting. It gives the average number of statements about forms or colours made by the members of the four groups during the course of the four series. The figures include statements such as "I saw something blue and something red between 8 and 12 o'clock," or "a triangular shape somewhere about 4 and 5 o'clock," if they corresponded to the facts of the slide, but not if they were wrong. It will be seen that the 'colour type' seems to have a wider field of view and therefore to notice more objects. By themselves the figures may not be absolutely convincing. Still, there is a considerable difference between an average of 26·6 for the F type and 37·0 for the C type. Also the figures for the mixed groups, 32·1 and 31·3, are intermediate between the extremes, as one would hope and expect. The differences are accentuated if we look at the records of spontaneous remarks or introspections. A very common remark of C type subjects is: "Goodness! My eye simply shot round that circle and I saw so many things that I can't remember them all." They actually do record shapes and colours in quite different parts of the field.

Table III A.

	% of subjects					
	Women			Men		
F	21·1	47·4		23·1	69·3	
FC	26·3		63·1	46·2		69·3
CF	36·8	52·6		23·1	30·8	
C	15·8			7·7		

Table III B. *All subjects.*

	% of subjects					
	Oeser			Scholl*		
F	21·9	56·3		26⅔	46⅔	
FC	34·4		65·7	20		40
CF	31·3	43·8		20	53⅓	
C	12·5			33⅓		

* *Zeitschr. f. Psych.* 1927, CI, 247.

Subjects of the F type, on the other hand, make fewer pronouncements, and these are nearly always about objects in the immediate neighbourhood of the Fp.

So far we have not gone beyond observed facts, and Lutz has made exactly similar observations. Lutz may also be right in regarding the reaction towards the Fp and the (apparently) narrow field of vision as both being ***stigmata*** of the F type, which he identifies with Kretschmer's schizothymic type. But it seems to us that when he attributes a restricted ***Auffassungsumfang*** to the F type, he is going beyond ascertainable facts, or rather, neglecting facts, in order to fall in line with the older classification of attention into wide and narrow, analytic and synthetic, etc[1]. We shall deal with the problem of attention and related questions in greater detail later, however, when the material has been brought under review.

Tables IIIA and IIIB show the percentage of subjects in the four groups. 47·4 per cent. of the women, and 69·3 per cent. of the men belong to the relatively form-dominant types; 52·6 per cent. of the women and 30·8 per cent. of the men to the relatively colour-dominant types. The percentages of 'mixed types,' FC + CF, are 63·1 and 69·3 respectively. On the whole, therefore, the women are more colour dominant. Lutz, Scholl and Poppinga have come to the same conclusion, without giving any figures, however. In Table IIIB our results are contrasted with those of Scholl, who used the same number of subjects. We had more form types, Scholl more colour types. Our percentage of mixed types was 65·7, that of Scholl 40·0. It seems likely that apart from differences in the presented material, racial differences are involved as well. This point will be discussed later.

Tables IV–VII contain a summary of all the results for the four series

Table IV. *Distribution, in %, of answers about the MF over the various series, I–IV, for each of the groups FC–C.*

Series	*FC*	*CF*	F	C	FC+F	CF+C
Women						
I. Fw Cw	11·0	9·4	35·3	32·0	15·6	16·3
II. Fw Cs	12·3	10·9	11·8	35·7	12·2	18·5
III. Fs Cw	52·1	39·1	35·3	14·3	48·9	31·5
IV. Fs Cs	24·7	40·6	17·6	17·9	23·3	33·7
Men						
I. Fw Cw	14·8	21·1	23·1	33·3	16·2	25·0
II. Fw Cs	14·8	18·4	15·4	27·8	14·9	21·4
III. Fs Cw	45·9	31·6	38·5	11·1	44·6	25·0
IV. Fs Cs	24·6	28·9	33·1	27·8	24·3	28·6
All subjects						
I. Fw Cw	12·7	13·7	30·0	32·6	15·9	19·6
II. Fw Cs	13·4	13·7	13·3	32·6	13·4	19·6
III. Fs Cw	49·3	36·3	36·7	13·0	47·0	29·1
IV. Fs Cs	24·6	36·3	20·0	21·7	23·8	31·8
% of total number of answers given (all series)	42·9	32·7	9·6	14·8	52·6	47·4

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[1] Cf. H. Henning, *Die Aufmerksamkeit.*

Table V. *Distribution, in %, of answers about the MF over the various groups, FC–C, for each of the series I–IV.*

Series	FC	CF	F	C	FC+F	CF+C
Women						
I. Fw Cw	27·6	20·7	20·7	31·1	48·3	51·7
II. Fw Cs	32·1	25·0	7·1	35·7	39·3	60·7
III. Fs Cw	52·1	34·2	8·2	5·5	60·3	39·7
IV. Fs Cs	34·6	50·0	9·6	5·7	40·4	59·6
Men						
I. Fw Cw	34·6	30·8	11·5	23·1	46·2	53·8
II. Fw Cs	39·1	30·4	8·7	21·7	47·8	52·2
III. Fs Cw	59·1	25·5	10·6	4·3	70·2	29·8
IV. Fs Cs	44·1	32·4	8·8	14·7	52·9	47·1
All subjects						
I. Fw Cw	30·9	25·5	16·4	27·3	47·3	52·7
II. Fw Cs	35·3	27·5	7·8	29·4	43·1	56·9
III. Fs Cw	55·0	30·8	9·2	5·0	64·2	35·8
IV. Fs Cs	38·3	43·0	7·0	11·6	45·2	54·7
% of total number of answers given (all series)	42·9	32·7	9·6	14·8	52·6	47·4

Table VI. *Distribution of F and C answers over the four series, for each of the four groups of subjects.*

		Answers	
Subjects	Series	FC+F	CF+C
F	I. Fw Cw	66·7	33·3
	II. Fw Cs	83·3	16·7
	III. Fs Cw	90·9	9·1
	IV. Fs Cs	73·9	26·1
FC	I. Fw Cw	50·0	50·0
	II. Fw Cs	53·8	46·2
	III. Fs Cw	67·9	32·1
	IV. Fs Cs	56·7	43·3
CF	I. Fw Cw	44·4	55·6
	II. Fw Cs	21·1	78·9
	III. Fs Cw	57·1	42·9
	IV. Fs Cs	20·0	80·0
C	I. Fw Cw	0	100
	II. Fw Cs	0	100
	III. Fs Cw	40·0	60·0
	IV. Fs Cs	0	100
FC+F	I. Fw Cw	56·3	43·7
	II. Fw Cs	68·0	32·0
	III. Fs Cw	76·8	23·2
	IV. Fs Cs	64·2	35·8
CF+C	I. Fw Cw	32·0	68·0
	II. Fw Cs	15·4	84·6
	III. Fs Cw	53·1	46·9
	IV. Fs Cs	15·2	84·8

Table VII. *Number of times both Fp and Cp were found.*

Series	No.	% of number of answers given in the series	% of total number of answers for all series
Women			
I	1	3·4	0·3
II	—	—	—
III	17	23·3	5·5
IV	6	11·5	1·9
Total	24	13·2	7·7
Men			
I	3	11·5	1·0
II	2	8·7	0·6
III	10	21·3	3·2
IV	6	17·6	1·9
Total	21	18·5	6·7
Total for all series, all subjects	45	—	14·4

Table VIII. *Distribution of F and C answers for series V.*

Subjects group	Series V (*a*)*		Series V (*b*)†	
	F	C	F	C
F	44·4	55·6	71·4	28·6
FC	22·2	77·8	81·8	18·2
FC+F	33·3	66·7	77·8	22·2
CF	15·4	84·6	71·4	28·6
C	0	100	50·0	50·0
CF+C	12·5	87·5	65·0	35·0
All subjects, all groups	23·5	76·5	71·1	28·0

* V (*a*): Cp=strong colour, weak form. Fp=strong colour, weak form. All other shapes in the same colour (yellow, weak).

† V (*b*): All shapes in the same colour (orange, weak), except *one strong form* (in a weak colour) and (one weak form) in a *strong colour.*

and the four groups into which the subjects were divided. It will be seen that not even in series III (weak colours, strong forms) did all the form-dominant subjects give 100 per cent. form reactions. The highest percentage of F reactions over the four series together is 90·9 (subject Ro). Some of the relatively pure F-dominant subjects gave 100 per cent. F answers for series III, but none of the C-dominant subjects. Table IV shows that of the total number of pure C reactions the largest proportion in the case of the women, is in series II (Fw Cs), whilst the men actually show a decrease in C reactions in series II. Again, there is a greater increase in F reactions in series III in the case of the women than in the case of the men, so that, on the whole, one might say that the women are more 'suggestible' and more variable in their reaction, more dependent on external factors.

Table VI shows this 'variability' even more clearly for the four groups. In group F, the increase of F reactions from series II to series III is relatively small (7 per cent.). There is a steady increase in F reactions between these two series as we proceed from group F to group C, the colour-dominant subjects[1]. Similarly, there is an increase of C reactions for the series with strong colours. The subjects that are most strongly influenced by a change from weak to intense colours or forms, are the subjects in the 'mixed groups,' above all in group CF. All these tables are averages, so that the finer individual differences are somewhat levelled up. If we look at the individual records, we find that subjects who give above 80 or below 20 per cent. F reactions are not susceptible to 'suggestion' by more intense forms or colours, at any rate within the limits of our series.

Scholl claims that the projection time of 200σ used by him made it impossible for his subjects to find both the Cp and Fp. Lutz mentions a few cases in which both poles were found. Table VII shows that even with exposures of 100σ both poles were found about once in seven times. There is practically no difference between men and women in this respect. The largest number of such cases was in series III, for two reasons. First, the subjects of the 'mixed' types CF, who are normally more colour than form dominant, were forced into F reactions by the strongly differentiated MF employed. But their natural reaction being more towards the Cp, they often found this as well. Secondly, the Cp was also associated with a strong form, so that the FC and F types, too, often found both poles.

As regards the quality of the observations in general, the F type is far more accurate than the C type. The latter made more mistakes, and were less accurate in localizing the seen forms or colours on the circle. They were also far less accurate in estimating the number of shapes actually present. Thus, many of the C type maintained to the end that there were from fourteen to eighteen coloured shapes, whilst F type subjects were inclined to underestimate rather than overestimate. When subjects of the C type had failed to find the MF or MC, it seemed to them that the time of exposure had been considerably shortened and that there were far more shapes in the circle. The 'clock method' of localizing also acted as a suggestion. Statements such as "twelve figures, one at each hour," were very common. Subjects of the F type, however, were hardly or not at all influenced in this way. In every way it was obvious that the C type reacted far more strongly to suggestion and were less critical. They were also far more emotional, and continually expressed their pleasure at success or annoyance at failure.

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[1] F, 7·6 per cent.; FC, 14·1 per cent.; CF, 36 0 per cent.; C, 40·0 per cent. In the case of the 'relative' F and C groups (*i.e.* F+FC, C+CF), the differences are 12·8 per cent. and 37·7 per cent. respectively (Table VI).

VI. GENERAL CONCLUSIONS. FORM AND COLOUR REACTORS AS PSYCHOLOGICAL TYPES.

Although the reactions of the majority of subjects to these experiments are not constant, there is a small number of subjects whose behaviour is constant, in spite of considerable changes in the presented material, and in spite of different suggestions or instructions. One type of subject is always impelled to select a given object from a number of different objects by its form, the other type by its colour. We say 'impelled' advisedly; for it is quite obvious to anyone who has performed these experiments, that there is no question of conscious selection according to one or other of the two possible modes of classification. In the first instance this is hardly possible because of the shortness of time during which the whole field is visible. Secondly, the behaviour of the subjects and their introspections show quite clearly that in each case the recognition of the form pole (Fp) or the colour pole (Cp) takes place as the result of an attraction that operates with reflex-like certainty and swiftness.

It might be argued that the results obtained in these experiments—the constant reaction to, or selection of, the Fp or Cp—is due to an assumed *attitude*, and not to an actual difference in the *sensations* experienced by the two types of subject. One could argue, for instance, that if in the projected field all the forms were equal, the subjects would have to select according to colour, and that if all the colours were equal, they would have to select according to form. But such an extreme simplification may be amusing from the point of view of drawing simple logical conclusions; from a psychological point of view, however, it is both useless and unnatural. Human experience and behaviour are not a matter of mechanical premises for syllogistic procedure. In actual everyday experience forms and colours are always associated in a great variety of ways. Every act of perception, it is true, involves a far-reaching negative abstraction of surrounding objects and processes; but it does not involve a completely one-sided abstraction within the perceived object as well.

In order to find out whether under extreme conditions one could still talk of 'form' and 'colour' reactions, two further series of experiments were carried out (series V (*a*) and V (*b*)). In the first series the main forms of Fig. 2 were again employed. The Cp was of a strong colour (red or green) and a weak form, the Fp also in a strong colour, different to that of the MF. All the other shapes in the field were weak forms, and all had the same weak colour (pale yellow). It is to be expected that all subjects would become strongly colour dominant. The results are given in the two left-hand columns of Table VIII. All C subjects gave 100 per cent. colour answers. Most of them found both the Cp and the Fp. But even when they found the Fp, it was merely because of its outstanding colour; *the MF was not recognized at all.* The F subjects, on the other hand, still gave 44·4 per cent. form answers and often did not see the Cp. The two right-hand columns show the results of series V (*b*). Here no specific instruction was given. The subjects were merely asked to describe what they had seen. Again, the field was of a uniform colour, except for two figures. One of these was a St Andrew's cross in pale green, the other a rectangle with two rounded corners in scarlet. Once more the dichotomy showed itself: 78 per cent. of the answers of the F subjects were form answers, while the C subjects gave 65 per cent. form answers, and correspondingly more colour answers.

These figures show clearly what all the other tables also show: the form (and colour) reactions are not due to a conscious attitude. They must be due to some more fundamental factor in the structure of the psycho-physiological organism, since they are within very wide limits unsusceptible to conscious control.

One of the subjects (F.C.B.) apparently adopted a particularly cautious and sceptical attitude. He was fully conversant with the aim of these experiments, and knew more or less what results had been obtained or were being expected. At the end of the session he remarked: "I have had some sad experiences in such experiments, in which I have always invented a great deal. Since then I have been very careful, and tend to see less than other people in similar circumstances. So I'm afraid I've not been of much use!" Probably because of his conscious attitude, he made more mistakes than anyone else, so that he was the worst observer even among the C-dominant subjects. Nevertheless he gave forty-eight answers in the first four series, the highest number scored, and 75 per cent. colour reactions. Another subject (K.G.P.), having heard us state that, on the whole, women were more colour dominant than men, determined to upset this pretty theory, and to be a perfect form reactor. 83 per cent. of her answers were colour answers!

These two examples demonstrate more clearly than many tables, that when the possibility of conscious comparison and selection is precluded, a previous attitude or voluntary decision has no influence on the primary attractive power of either form or colour. In some way, therefore, form and colour are selective principles operative in the psycho-physiological organism to a different degree in different people. It becomes relevant to enquire whether in this reflex-like attraction towards either form or colour we have a means of distinguishing structurally different psychological types[1].

The writer's thanks are due to Prof. F. C. Bartlett for help and advice, to Prof. E. R. Jaensch for allowing him to look at the work of Poppinga and Ritter in Marburg, and to Sergeant T. Ellcock, R.A.F., for constructing the tachistoscope.

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[1] The analysis of the temperamental characteristics was made by means of Rorschach's tests (Hermann Rorschach, *Psychodiagnostik*, Bern and Leipzig, 1921). In a following paper the results of these tests will be given and discussed, together with a critical analysis of the tests themselves. Also the bearing of these results and of the form-colour dominance tests on the theory of perception and of selective principles will be considered.

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INDIVIDUAL DIFFERENCES IN PHENOMENAL REGRESSION.

By ROBERT H. THOULESS.

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  (*a*) *Intelligence* (pp. 234–235).
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I. METHOD AND AIM OF THE EXPERIMENTS.

IN earlier papers (1, 2) the tendency of subjects to see the shapes of inclined objects, the relative sizes of similar objects at different distances, the relative brightnesses of objects of different reflectivities under different illuminations, etc., was referred to a tendency for the object of immediate experience (the 'phenomenal' object) to have characters intermediate between those given in peripheral stimulation ('stimulus' characters) and the characters of the 'real' object. This tendency was called the tendency to "phenomenal regression to the 'real' object."

This general principle leads immediately to problems in individual psychology. Do all subjects show this tendency, even those who have studied and practised the principles of perspective and who generally believe themselves to be free from it? Do all subjects show it to the same extent, and, if not, how is its amount in a given individual related to other measurable characteristics of the individual? Are the different measurements of phenomenal regression quantitatively related so that an individual showing a large amount of it in one test will tend also to show a large amount in another? Will, for example, the individual subject who sees an inclined circular disc as almost a true circle be likely also to see the larger of two discs as the apparently larger even when it is very much further away than the other?

In order to answer these questions, it is necessary to administer different tests of phenomenal regression to a large number of subjects under identical conditions. The tests chosen were (*A*) a test of the tendency to constancy of shape, (*B*) a test of the tendency to constancy of size, and (*C*) a test of the tendency to constancy of brightness.

The test *A* of tendency to constancy of shape was administered with the apparatus shown in Fig. 1 of an earlier paper (1). The object was a white circular disc of diameter 39·75 cm. lying flat on the table with its centre at a distance of 129 cm. from the base of a screen through which the subject looked at the disc from a height of 48·5 cm. above the table. The inclination of the disc from the line of vision to the centre was thus 20° 36′. This produced a retinal image of the same shape as would an approximately elliptical figure with ratio of short to long axis of 0·36 : 1. A set of white cardboard ellipses was prepared of axis ratios ranging from 0·30 : 1 to 1·00 : 1 by steps of 0·05 : 1. A sufficient number of these were presented serially to the subject at right angles to his line of vision, and he was asked to report whether the presented ellipse looked 'fatter than,' 'thinner than' or 'the same shape as' the circle appeared from that point of view. It was emphasized that the question was about shape (*i.e.* axis-ratio) not size, and that the experimenter wanted to know not the real shape of the disc (which the subject saw and had been told was circular) nor how he supposed it ought to look, but simply how it did look to him. In order to make quite certain that the subject understood these instructions, a preliminary experiment was done in which the object was an inclined ellipse with long axis pointing in line with the subject, and the axis-ratio was changed until the subject said that the object looked circular.

In the proper test with the circular object, a few preliminary observations were made beginning with an ellipse which elicited the response 'thinner' and increasing the axis-ratio continuously until several 'fatter' responses had been given, followed by a series in the reverse order. A few preliminary series were given until the subject's responses were fairly constant. Then three ascents and descents were made and the mean of the subject's six equal responses was taken as the phenomenal shape of the inclined circle for him.

The ellipse chosen as representing the apparent shape of the inclined circle was never the one corresponding in axis-ratio to the perspective projection of the circle, but was always intermediate between this and the circle, showing that in all cases the phenomenal shape was a compromise between stimulus shape and the 'real' shape of the object observed.

For test *B*, a white circular disc (of diameter 39·7 cm.) was supported at right angles to the table and to the subject's line of vision at a distance of 230 cm. from his eyes. A smaller disc (of 29·7 cm.) was arranged so that the subject saw it to one side of the larger disc and separated from it (Fig. 1). The distance of the smaller disc from the subject's eyes was adjusted until he reported that the two discs looked equal in size. Preliminary practice and method (of ascent and descent) were as in test *A*. The instructions were: "That is a large disc and this is a small one. This one looks smaller than the other, doesn't it? (holding it at the same distance from the subject as the large one). As I bring it nearer to your eyes it looks bigger and bigger, until finally it looks bigger than the other one (bringing it to within a short distance of the subject's eyes). Now I want to discover the exact position at which this disc looks the same size as the other." The object of the instructions is not, of course, to suggest a result to the subject (the phenomenon of changing apparent size with distance occurs without any instructions), but to make clear to him that it is apparent size that he is to report on.

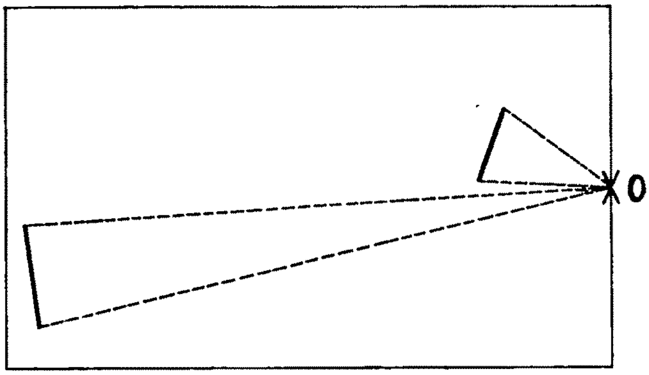

Fig. 1. Method for Test *B*.

The distance at which the retinal image of the smaller disc was equal to that of the larger was 172 cm. At this point, all subjects saw the smaller disc as much smaller than the other. In order that they might be seen as equal it was necessary to bring the smaller one much nearer to the eyes.

Phenomenal size was thus also a compromise between stimulus size and 'real' size.

This is not an ideal method of experimenting, and difficulties were found in the administration of test *B* as the series of experiments went on. A better method would have been to keep the distances constant and to present as variable a series of discs of different diameters.

The most serious difficulty in the method actually adopted was that individual differences were so large that the small disc used for most subjects could not be used at all for those subjects who showed great phenomenal regression, since there was no measurable distance from the eyes at which it did not appear the smaller. If this disc still appeared the smaller at a distance of 30 cm. (nearer than which led to difficulties of accommodation) it was replaced by one of diameter 32·2 cm. If this still looked the smaller when its distance was reduced to 30 cm., it was replaced by a disc of diameter 34·7 cm. Finally, if this proved too small, it was replaced by a disc of 37·7 cm. (only 2 cm. less in diameter than the standard disc).

It was obviously unsatisfactory that different subjects were tested by different discs. Since larger discs were only used when the subject's amount of phenomenal regression was too great for the smaller ones, this did not in any way alter the ranking of the subjects, but introduced an element of uncertainty in the relative values of the numerical indices assigned to different subjects as a measure of their amount of phenomenal regression. This might have so disturbed the results of test *B* as to obscure or at least seriously reduce its correlations with other tests. Since, in fact, test *B* proved to correlate with other tests as well as did test *A*, the effect of this substitution of larger discs proved to be practically unimportant.

Test *C* was simply a reproduction in quantitative form of Hering's original experiment on 'gedächtnissfarben.' The arrangement of the experiment was as for test *B*, except that, instead of two white discs of different size at controlled distances from the subject's eyes, there were two discs of different reflectivities at controlled distances from the source of light. The discs used were cut from Zimmermann's series of 50 grey papers, one being the standard white, the other No. 13 grey, which proved on measurement to have a reflectivity 0·228 that of the white. The sole source of light in the room was a 4-volt, 3·1 C.P., electric bulb at 290 cm. from the white disc. The subject sat behind the lamp.

The instructions were: "That is a white disc, this is a grey one. When I hold it here (holding the grey disc well behind the point at which its luminosity equals that of the other) it looks less bright than the other.

As I bring it nearer to the lamp, it looks brighter and brighter, until finally it looks brighter than the other one. Now I want to discover the exact position at which it looks the same brightness as the other." This point was then determined in exactly the same way as the point of equal apparent size in test *B*.

In this test the point at which the two papers were of equal luminosity, *i.e.* at which they were equivalent as stimuli, was when the grey was 138·5 cm. from the lamp. In nearly all cases, the grey at this point looked much the darker, and was only seen as of equal brightness with the other when it was brought much nearer to the light. Unlike the results of the other two experiments, there were a few exceptions to this rule, three subjects balancing for brightness only when the grey paper reflected less light to the eyes than the other. The explanation of these exceptions will be discussed later.

The results used were drawn from a total number of 127 subjects. These were grouped as follows:

Group M. 42 male university students of ages ranging from 19 to 23. For 38 of these, I also had intelligence quotients derived from the Cattell test, and for 28, results of an N.I.I.P. group test[1].

Group N. 27 female university students of the same age range, of whom 26 had done the Cattell test and 23 the N.I.I.P. test.

Group O. 36 male subjects of ages ranging from 24 to 54½.

Group P. 12 male students of the Glasgow School of Art of the same age range as group *M*.

Group Q. 10 artists and teachers of art[2].

The subjects of the first three groups were tested in all three tests. Most of the subjects of groups *P* and *Q* were given only tests *A* and *B*.

The aims of the investigation were as follows:

(1) To discover how great was the range of individual difference in the seen shapes of inclined objects, of the seen sizes of objects at different distances and of the seen brightness of papers of different reflectivity under different intensities of illumination.

(2) To discover whether the tendency to phenomenal regression in these three tests was dependent on a group factor common to all.

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[1] For permission to use the results of the Cattell test, I am indebted to Dr Inglis and Mr Alexander of the Glasgow University Education Department, and for the use of the results of the N.I.I.P. test to Mr Oakley of the Scottish branch of the N.I.I.P.

[2] For access to groups *P* and *Q*, I am indebted to the kind co-operation of Mr Mainds of the Glasgow School of Art.

(3) To discover how the tendency to phenomenal regression was related to intelligence, sex, age and temperament.

(4) To discover whether training or practice in painting and drawing decreased the tendency to see objects in their 'real' shapes and sizes.

The main purpose of the investigation was the solution of the problems (1) and (2). Those mentioned under (3) were secondary questions solved so far as adequate data for their determination was provided in the course of the investigation. For this reason, some of the answers to the problems in group (3) were inconclusive and in no case did they possess a high degree of significance. For more satisfactory solution of these problems a more extended investigation would be necessary directed towards these problems alone.

II. UNIVERSALITY AND RANGE OF PHENOMENAL REGRESSION.

Of the whole number of 127 subjects tested, every one showed phenomenal regression in greater or less degree for tests *A* and *B*. It is important to notice that this number includes the group of artists and art students, since it has been suggested that the effect described is only to be found in children or in those who have not been taught the laws of perspective or in those who follow exotic traditions of drawing. The group of ten artists included both those who practised perspective drawing and those who did not. It happened that one of this group, who was a teacher of the principles of perspective, showed actually a larger amount of phenomenal regression than a well-known artist who has deliberately rejected perspective drawing. This rule of universality of the effect does not, as we have seen, hold for test *C* as described above. Results to be described later, however, suggest that this is probably not an exception to the general rule of the tendency of all subjects to show phenomenal regression to the character of the 'real' object, but to a defect in this method of carrying out the experiment.

Katz (3) has reported considerable individual differences in the 'brightness' experiment. This is fully confirmed. In all three tests the range of differences between individual subjects was very large. Figs. 2 and 3 show median and extreme results for the 105 subjects of groups *M*, *N* and *O* in tests *A* and *B* respectively[1]. In these diagrams, the convention used earlier of representing the stimulus character by a black figure, the 'real' character by a broken line, and the phenomenal character by a continuous line has been adopted. Fig. 2 shows that the subject with least phenomenal regression sees a figure little wider than the stimulus ellipse, while the subject showing most phenomenal regression sees the figure as little different from a circle viewed normally. This subject, in fact, reports that the apparent shape is practically circular, and even when the angle of inclination is much increased, he says that it does not look very different from a circle.

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[1] Comparison of these diagrams with Figs. 2 and 5 of (1) shows that the subject S. used in the earlier investigation happened to have a considerably smaller tendency to phenomenal regression than the median. This fact does not affect the validity of the conclusions drawn earlier, but the quantitative results are not representative of the average subject. Fig. 6 (1), for example, for the average subject, would be much nearer to a horizontal line. This figure shows an unusually small tendency to constancy of size with increasing distance of object.

The extremely large difference in what is seen by different subjects looking at the same object under identical conditions is a very striking result of these experiments. It is also to be noticed that when the effect is subsequently demonstrated to them, the surprise of subjects at the effect tends to be in inverse proportion to its amount. Subjects showing little phenomenal regression are astonished to discover that their perceptions are not exactly determined by the stimulus characters of the objects they are looking at. The divergence between phenomenal and stimulus character, although considerable, has not forced itself on their attention, and they have formed a habit of supposing that the two were identical. Subjects with great phenomenal regression, on the other hand, tend to show no surprise and little interest in the result. Apparently the divergence between stimulus and phenomenon is for them so large that they have never formed the habit of supposing that they were identical.

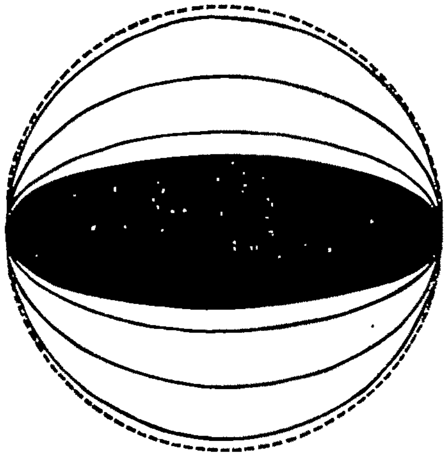

Fig. 2. Tendency to constancy of shape. (Median and extreme values.)

Table I shows numerically the median, quartile and extreme values amongst the 105 subjects of these groups (104 only for test *C*, since one subject did not do this test). Table IA shows the result for test *A*. The numerical quantity given is the ratio of the shorter to the longer ellipse axis. The results for *B* and *C* are shown in Table 1B. It is necessary to present these in somewhat different form, since in these it was the phenomenal and not the stimulus character that was kept constant. For *B* the quantity shown is the ratio of diameter of the small to that of the large ellipse; for *C*, the ratio of brightness of the grey paper to that of the white.

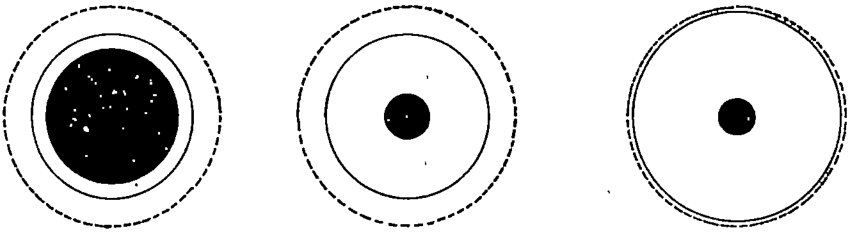

Fig. 3. Tendency to constancy of size.

This table is intended merely to show the great range of individual differences. It is not intended to establish norms, since the individuals tested were by no means representative of the population as a whole, particularly with respect to intelligence.

In order to have a convenient figure to compare amounts of phenomenal regression in different subjects and in different tests, an index was calculated which would vary from the value zero if the subject showed no phenomenal regression and saw the stimulus character, up to unity if he showed complete phenomenal regression and saw the 'real' character. This measure (called the 'index of phenomenal regression') was

$$(\log P - \log S)/(\log R - \log S),$$

where *P* is the numerical value of the phenomenal character, *S* that of the stimulus character and *R* that of the real character. It is obvious that this index is arbitrarily chosen for reasons of practical convenience (explained in article (1)), and has no theoretical validity. It must also be remembered that the absolute value of the index for any individual is strictly relative to the conditions of the experiment in which it was obtained, and would be different for other conditions of experimentation. Our present purpose is only to compare degrees of regression for individuals doing the same test under identical conditions. The corresponding index of regression is shown in brackets under each entry in Table I.

Table I A.

Test	Numerical value of 'real' character	Numerical value of 'phenomenal' character					Numerical value of 'stimulus' character
		Lowest	First quartile	Median	Last quartile	Highest	
A	1 : 1	0·45 : 1 (0·225)	0·64 : 1 (0·57)	0·70 : 1 (0·655)	0·78 : 1 (0·76)	0·95 : 1 (0·95)	0·36 : 1

Table I B.

Test	Numerical value of 'real' character	Numerical value of 'phenomenal' character	Numerical value of 'stimulus' character				
			Lowest	First quartile	Median	Last quartile	Highest
B	0 748 : 1	1 : 1	1·22 . 1 (0·40)	2·05 : 1 (0 71)	3·44 : 1 (0·81)	—	—
	0·811 : 1	1 : 1	—	—	—	3·27 : 1 (0·85)	—
	0·950 : 1	1 : 1	—	—	—	—	8·55 : 1 (0·98)
C	0 228 : 1	1 : 1	0·746 : 1 (negative)	1·41 : 1 (0·19)	1·92 : 1 (0·305)	3·71 : 1 (0 47)	17 24 : 1 (0·66

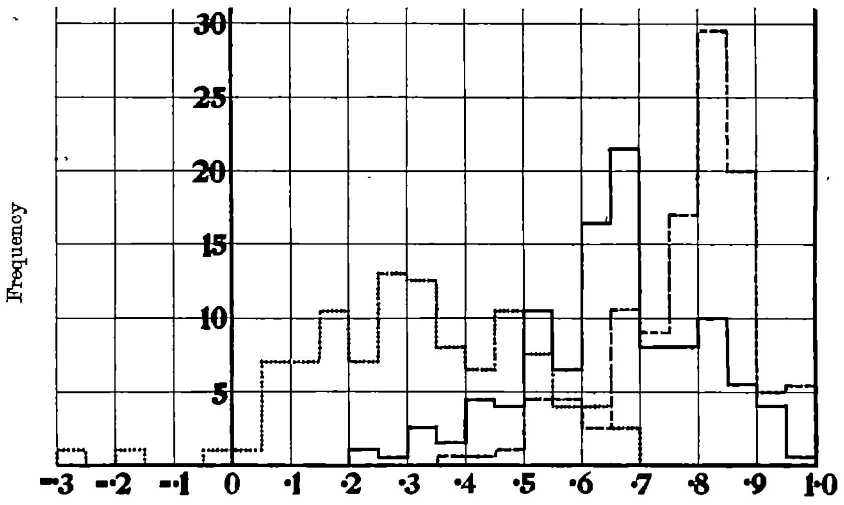

Fig. 4. Indices of phenomenal regression.

———— Test *A*. — — —— — Test *B*. ········ Test *C*.

Fig. 4 shows the frequency distribution of the indices of phenomenal regression for tests *A*, *B* and *C*, given by the subjects of groups *M*, *N* and *O*. This diagram shows that the distribution is not on a normal curve. One reason for this is the same as the reason for the lack of normality of the frequency distribution of correlation coefficients: the index has a maximum value of unity, so that asymmetry is produced by crowding together of the higher values. This asymmetry is greatest for test *B*, since its median value is nearest to unity.

It is possible that some other measure of the regressive tendency might have made the distributions more closely normal. While a fairly simple transformation of the measure was found which reduced the curve of *A* to normality, I did not succeed in finding any measure which made all three curves normal.

We are concerned only with the practical problem of whether the deviation from normality is too great for the validity of the statistical operations it is required to perform on these figures. These are: (*a*) finding correlations and their standard errors by the product-moment method, and (*b*) estimating the significance of the differences between two means from the standard error of each mean.

It is usual in statistical work to treat any distribution of a 'cocked hat' form by the methods appropriate to the normal distribution if the true distribution is not too far removed from normality. If the distribution differs not too widely from the normal curve, this introduces a small but not exactly calculable element of uncertainty into the results obtained. This element of uncertainty is usually ignored if it is small.

We can apply a few empirical tests of how far treatment of these figures as if they were normally distributed misrepresents the important facts of the distribution.

First, we may calculate the standard deviation for each test and find the number of individual cases falling within the limits of the mean ± P.E., ± 2 P.E., and ± 3 P.E., comparing these observed numbers with those to be expected in a normal distribution. The result is shown in Table II. It will be seen that the observed number is not widely different from that indicated by expectation from a normal distribution.

Secondly, we may examine the distribution of a set of means obtained from one of the sets of test results. The usual method of estimating the significance of differences depends on the fact that the means of

Table II. *Comparison of observed distributions of indices of regression with expectation in normal distribution.*

				No. within mean ±0·6745 S.D.		No. within mean ±2 P.E.		No. within mean ±3 P.E.	
est	No. of cases	Mean	S.D.	Observed	Expected	Observed	Expected	Observed	Expected
A	105	0·655	0·149	54	52·5	85	86·5	103	100·5
B	105	0·779	0·119	56	52·5	88	86·5	99	100·5
C	104	0·325	0·176	44·5	52	86	85·5	102	99·5

values are very commonly normally distributed even when the values themselves are not, and that the standard deviation of the means is calculable from the standard deviation of the values themselves (if these last are approximately normally distributed) by the formula $\text{S.D.}_M = \sigma/\sqrt{N}$.

To test the normality of the means, a random grouping five at a time was made of the 105 values of the index for test *B* given by groups *M*, *N* and *O* together. *B* was chosen since it was the least normal distribution and therefore the least favourable for showing normality in its means. The usual calculation from the third and fourth moments shows that the distribution of these 21 means is not significantly different from normal. All means used in later calculations are from more than five values and will therefore be less likely to differ from normal distribution. The observed standard deviation of these means was 0·049 (S.E. 0·0076), while that calculated from the standard deviation of the group as a whole was $0{\cdot}119/\sqrt{5} = 0{\cdot}053$ (S.E. 0·0037). The difference is 0·004 with standard error of 0·0085. Prediction of the distribution of means from that of individual values made by treating these as normally distributed is not, therefore, appreciably in error even in the most unfavourable case of test *B*.

It seems, therefore, that we can use methods of calculation appropriate to the normal distribution for these results without any considerable error. It is possible, however, to check certain results by also applying methods which do not assume normality of distribution. For estimating the significance of differences of means, we can, for example, make a fourfold table about the median of the combined set of values and estimate the significance of any difference between the partitions of the two sets by making use of the χ^2 distribution. For correlations we can also use the rank-difference formula, which, although it is not generally regarded as a method as satisfactory as the product-moment method, has the advantage of being independent of the particular measure used. It is, in fact, an estimate of the correlation which would be obtained if the method of measurement chosen were such as to give a normal distribution of the quantities measured.

III. THE EVIDENCE FOR A GROUP FACTOR.

Thirty-eight of the group *M* of 42 male university students had done the Cattell test of intelligence (which we may call *I*). The intercorrelations (calculated by the product-moment formula) of the tests *A*, *B*, *C* and *I*, are shown in the upper right half of Table III. The figures in italics in the lower left half show partial intercorrelations of *A*, *B* and *C*, with *I* eliminated. As a check on these figures, Table IV shows the results obtained by the rank difference method of calculation. While the absolute amounts of correlation are somewhat different in the two tables, the main results are the same. There is a considerable correlation between the results of tests A and B even with I eliminated, but the correlations of C with A and B are insignificant.

Results with the 26 female subjects of group N who had done the intelligence test are the same in their general indications, although r_{AB} is smaller (but still significant), while r_{AC} is negative (and still insignificant).

Table III. *Total and partial correlations (by product-moment method) obtained from* 38 *male students, with standard errors (in brackets).*

	A	B	C	I
A	—	+0·695 (0·085)	+0·085 (0·165)	−0·15 (0·16)
B	*+0·69 (0·085)*	—	+0·18 (0·155)	−0·30 (0·15)
C	*+0·055 (0·165)*	*+0·12 (0·16)*	—	−0·22 (0·155)

Table IV. *Total and partial correlations from the same* 38 *students calculated by rank-difference method and converted into corresponding r's, with standard errors.*

	A	B	C	I
A	—	+0·63 (0·095)	+0·12 (0·17)	−0·06 (0·17)
B	*+0·65*	—	+0·33 (0·15)	−0·34 (0·15)
C	*+0·105 (0·17)*	*+0·25 (0·16)*	—	−0·31 (0·155)

The absence of significant correlation between test C and the other two tests will be discussed in the next section. The positive result which has so far emerged is the presence of a considerable correlation between the tests of the tendency to phenomenal regression for shape and for size which is not dependent on the correlation of each of them with the intelligence test. If the intelligence test is a sufficiently reliable measure of G (the general factor), this is equivalent to the statement that there is a group factor of phenomenal regression independent of G.

The intercorrelations of A, B and I are, of course, all subject to 'attenuation' by random errors in the individual values. In view of the largeness of the partial correlation between A and B after I has been eliminated, it is unlikely that the same result would not hold for unattenuated coefficients. To make the matter more certain, however, we may adopt one of two methods: (*a*) make corrections for attenuation in all the correlation coefficients used, or (*b*) use some criterion for the presence of a group factor into which the effect of attenuation does not enter. The first of these two methods is laborious and (since formulae proposed for the correction of attenuation are not universally accepted by statisticians) also uncertain. The second method is therefore adopted. Spearman's criterion that the 'tetrad difference,' $r_{WX}.r_{YZ} - r_{WY}.r_{XZ}$ will be greater than zero if W and X (or Y and Z) have a factor in common which is not also common to the pairs WY and XZ has been shown by him to be true of attenuated coefficients if it is true of unattenuated coefficients. It can, therefore, be used with correlation coefficients subject to the effects of attenuation.

For the use of this criterion, the results of a second test were used. This was a test partly of intelligence and partly of information which will be described in future as test O. An insufficient number of group M had taken this test as well as test I, so, for this purpose, results from groups M and N were taken together. In all 27 of group M and 26 of group N had taken both tests I and O, giving a total of 53.

There was a small sex difference in the tests A and B, so if the combined results of both groups had been used as if they came from a homogeneous group, the correlation between A and B would have been slightly increased through the association of A and B with sex. This effect was eliminated by the calculation of all deviations not from the mean of the combined group but from the mean of the sex group to which the individual belonged, *i.e.* the formula used was

$$r = \frac{\Sigma\,(X_M - \bar{X}_M).(Y_M - \bar{Y}_M) + \Sigma\,(X_N - \bar{X}_N).(Y_N - \bar{Y}_N)}{\sqrt{\Sigma\,(X_M - \bar{X}_M)^2.\Sigma\,(Y_M - \bar{Y}_M)^2 + \Sigma\,(X_N - \bar{X}_N)^2.\Sigma\,(Y_N - \bar{Y}_N)^2}},$$

where X_M, $Y_M + \bar{X}_M$, $\bar{Y}_M$ stand for individual values and for means respectively of the two correlated tests for the group of men, and X_N, Y_N, and $\bar{X}_N$, $\bar{Y}_N$ those for the group of women.

The effect of the sex difference on the estimated value of the correlation is, in any case, very small. The correlation between A and B, for example, calculated by the above formula is 0·65; if it had been calculated by treating the whole group as homogeneous, it would have been 0·665.

The intercorrelations of A, B, I and O for this group of 53 subjects are shown in Table V. The tetrad difference can be calculated in two ways giving somewhat different results:

$$(a)\ r_{AB}.r_{IO} - r_{AI}.r_{BO} = 0{\cdot}3635 - 0{\cdot}138 = 0{\cdot}2255;$$
$$(b)\ r_{AB}.r_{IO} - r_{AO}.r_{BI} = 0{\cdot}3635 - 0{\cdot}0845 = 0{\cdot}279.$$

The calculation of the standard error of this quantity presents some difficulty, since there is not complete agreement as to its amount by statisticians. I have used the formula which is given by Spearman (4) as the best measure of the standard error for a small number of tetrads[1]. This gives a standard error of 0·115 for tetrad (*a*) and 0·097 for (*b*). The probability of the occurrence of these by the chances of sampling is 0·05 for (*a*), but for (*b*) is less than 0·01. While the result for (*a*) alone would only enable us to assert probable significance, that for (*b*) definitely significant. The tetrad equation, therefore, indicates the presence of a group factor.

This confirms the result previously indicated. Tests *A* and *B* have a group factor in common, independent of *G*, which we may call the tendency to phenomenal regression (or P.R.). This means that different individuals have different degrees of tendency to regress to the real character of the object in perception and that the degree of the P.R. tendency in a given individual determines both his tendency to see objects in their real shapes and also in their real sizes. It is not yet proved whether it also determines his tendency to see them in their 'real' brightnesses.

Table V.

	A	*B*	*I*	*O*
A	—	+0·65	−0·31	−0·32
B	+0·65	—	−0·265	−0·445
I	−0·31	−0·265	—	+0·56
O	−0·32	−0·445	+0·56	—

IV. THE DISTINCTION BETWEEN 'WHITENESS' AND 'BRIGHTNESS.'

An unexpected result of the correlations of the three measures of phenomenal regression was that, whereas there was a high correlation between the results of the tests *A* and *B*, the correlation of both of these with *C* was small or insignificant, although the tendency to constancy of brightness would seem to psychological analysis to be of the same nature as the tendency to constancy of shape or of size. Is the psychological function involved in the test *C* really different from that of *A* and *B*, or is there some complication in the measurement of *C* which might be expected to obscure its relationship to *A* and *B* if such a relationship really existed?

That there is such a complication in the measurement of the tendency to constancy of brightness is apparent after the experiment has been carried out a few times. The average subject who has complained of no special difficulty in giving unambiguous answers to what is asked of him

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$$\sigma^2=\frac{1}{N}\left[r_{WX}^2+r_{YZ}^2+r_{WY}^2+r_{XZ}^2-2\,(r_{WZ}r_{WX}r_{XZ}+r_{WZ}r_{WY}r_{YZ}+r_{WX}r_{WY}r_{XY}+r_{XZ}r_{YZ}r_{XY})+4r_{WX}r_{YZ}r_{WY}r_{XZ}\right].$$

in experiments *A* and *B*, spontaneously raises a difficulty soon after the beginning of experiment *C*. The form of his objection will vary with the adequacy of his vocabulary, but a common form of it is: "Do you want me to tell you when they look equally bright or when they look equally white. They look the same brightness now, but the nearer paper (*i.e.* the physically grey paper), still looks greyer than the other one." There is thus, for most subjects, a point at which the papers look equally 'bright,' although one looks a grey and the other an equally bright white. If the illumination of the less reflective (the grey) paper is still further increased, a point can be obtained at which both papers look equally white, although the grey paper appears the brighter. If this is brought still nearer to the light, it looks both 'brighter' and 'whiter' than the other.

Table VI shows, for example, the luminosity of the grey paper relative to that of the white for the phenomenal equality of 'whiteness' and 'brightness' for the first eight of group *N*. It will be seen that there are wide individual differences. To be seen as equally white, the grey paper may have to reflect more than ten times the amount which made it appear equally bright. For some subjects on the other hand, it may have to reflect no more; they see the two papers as of identical shade[1] when they are balanced for brightness and they call the appearance of both 'grey.'

Table VI. *Sample of results showing difference between points of balance for whiteness and for brightness.*

Subject	Ratio of luminosities for equality of phenomenal brightness	Index of P.R. (for brightness)	Ratio of luminosities for equality of phenomenal whiteness	Index of P.R. (for whiteness)
*N*1	2·92 : 1	0·42	10·87 : 1	0·62
*N*2	1·37 : 1	0·175	18·7 : 1	0·665
*N*3	4·99 : 1	0·52	5·41 : 1	0·535
*N*4	14·37 : 1	0·645	29·5 : 1	0·695
*N*5	2·08 : 1	0·33	5·60 : 1	0·54
*N*6	1·92 : 1	0·305	7·82 : 1	0·585
*N*7	4·27 : 1	0·495	4·27 : 1	0·495
*N*8	4·68 : 1	0·51	53·5 : 1	0·73

If there were a real relationship between the results of test *C* and those of the other tests, but this relationship was prevented from appearing in the correlation results by some effect of the complication introduced by the double balance point in test *C*, it would follow that a high correlation should be found if we used only the results drawn from those subjects who show identity of the points of balance for whiteness and for brightness, in whom, therefore, the complicating factor is absent.

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[1] Although they may see a slight difference of hue between them, the white paper looking very slightly yellower. This is, no doubt, due to the grey paper used being actually of slightly different hue from the white. To most subjects (and to myself) this difference is inappreciable, being masked by the much greater difference between their phenomenal 'whiteness.'

Unfortunately, the commonness of the phenomenal difference between brightness and whiteness, and the fact that I experience this difference myself, made me suppose that there were no persons who did not experience the difference. It was not, therefore, until the series of experiments was more than half completed, that I began to measure both balance points for all subjects. Of 45 subjects so examined, 10 only were found to whom the position of equal phenomenal brightness was also the point of equal phenomenal whiteness. This is a number too small for a significant determination of a correlation.

Of the remaining 35 subjects, however, some show a large, some a small separation between the two points of balance. The whole number can be divided into two groups: 22 in whom the ratio of the index of regression for whiteness to that for brightness is either unity or an amount less than 1·35, and 23 in which it exceeds this amount. If our hypothesis is correct, the first of these groups will show high, the other low correlation with the results of the other two tests.

The correlation of the indices of regression for brightness with the mean index for size and for shape is found to be 0·45 (S.E. 0·17) for the first group and only 0·18 (S.E. 0·21) for the second[1]. The number of cases is still small, but the first of these figures exceeds its estimated standard error by a sufficiently large amount to enable us to say with a fair degree of confidence that the correlation between the results of test *C* and the combined result of the other two tests is very much higher for those subjects who show small divergence between phenomenal brightness and whiteness than for those showing a large difference, thus confirming our suspicion that this divergence was a complicating factor obscuring the dependence of phenomenal regression for brightness on the group factor already distinguished.

We may next ask whether it is the index of phenomenal regression for brightness, that for whiteness, or the mean of these two, that gives the best correlation with the results of the other two tests, and is, therefore, to be judged the best measure of phenomenal regression. For this purpose, we can use correlations obtained from all 45 subjects, and we find that these give a clear answer to the question. The correlation of $(A+B)/2$ with the index for brightness is only 0·24 (S.E. 0·15), while that of $(A+B)/2$ with the index for whiteness is 0·58 (S.E. 0·105). It is the index from phenomenal equality of whiteness and not of brightness which should have been measured to give a true indication of the subjects' tendency to phenomenal regression. The correlation of the mean of these measures with $(A+B)/2$ is less than that of the index for whiteness alone.

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[1] This and all other correlations in this section were obtained by the rank-difference method. Twenty-six of the 45 were from group *N*, 3 from *M*, 12 from *O*, 2 from *Q* and 2 unclassified. For the present purpose of comparing correlations and not determining their absolute values, the small effect of this heterogeneity does not matter.

Even more striking is the fact that for the 23 cases of large difference between brightness and whiteness balance, which gave insignificantly small correlations between $(A+B)/2$ and the index for brightness, the correlation of $(A+B)/2$ with the index for whiteness is not significantly different from that for the whole group of 45 cases. We may conclude, therefore, that when there is a large difference between the point of balance for whiteness and for brightness, the latter shows little relationship with the index of regression as measured in other ways, while the index of regression for whiteness shows no disturbance in its closeness of relationship.

The results given above are summed up in Table VII.

Table VII.

	Whole group of 45 subjects	Group of 22 subjects showing small difference between phenomenal brightness and whiteness or no difference	Group of 23 subjects showing great difference between phenomenal brightness and whiteness
Correlation between $(A+B)/2$ and index of regression for brightness	0·24 (S.E. 0·15)	0·45 (S.E. 0·17)	0·18 (S.E. 0·21)
Correlation between $(A+B)/2$ and index of regression for whiteness	0·58 (S.E. 0·105)	—	0·60 (S.E. 0·14)
Correlation between $(A+B)/2$ and mean of indices of regression for whiteness and for brightness	0·46 (S.E. 0·12)	—	—

Unfortunately, an insufficient number of this group of subjects had also taken the intelligence test for it to be possible to obtain a significant partial correlation of $(A+B)/2$ with the index of regression for whiteness. That our conclusion would be supported by this criterion also is sufficiently certain from the fact that if the correlation previously obtained between $(A+B)/2$ and I is reasonably correct then even complete negative correlation between I and the index of regression for whiteness would not produce the observed correlation between $(A + B)/2$ and the index for whiteness unless there were a considerable partial correlation between these. Actually, of course, it is probable that the correlation between I and the index of regression for whiteness is of the same order as that between I and the other tests, in which case the partial correlation would be larger.

We may conclude that the results of test C are also dependent on the group factor (which we have called P.R.) that determines the tendency to see the real shape and the real size of an object. The effect of a large P.R. factor in a given subject is, however, not to make the apparent brightness but the apparent whiteness of a colourless object approximate to the 'real' character of reflectivity.

When we ask a subject to report on apparent brightness there is opposed to this tendency to phenomenal regression to the 'real' character of reflectivity a greater or less tendency in the opposite direction. The most probable explanation of this appears to be that apparent 'brightness' means in part to a subject 'intensity of illumination.' What he calls 'whiteness' is the phenomenal character corresponding to the real character of reflectivity, while 'brightness' is a phenomenal character partly corresponding to the 'real' character of reflectivity and partly to the 'real' character of intensity of illumination. If this is the true explanation, it explains both anomalies in the 'brightness' form of test C: its low correlation with the results of the other tests, because phenomenal regression is here acting in both directions and therefore producing very varied quantitative results; and also the occasional lower luminosity of the grey paper at the point of balance for brightness, because the point at which they are equally illuminated is where the two papers are equidistant from the light. No case was discovered in which the point of balance for whiteness was one at which the grey had lower luminosity (*i.e.* showing a negative index of regression), although one of the 45 cases was one of the few showing this anomaly for the brightness balance. It seems likely that all three cases showing a negative index of regression for brightness were simply cases of considerable difference between whiteness and brightness, and would have shown a positive index for whiteness. Unfortunately this could not be tested for the other two.

A last point of interest arises from the results described in this section. It is probable that many differences between psychological theories arise from individual differences between psychologists. One such possibility of individual differences is shown here. Some psychologists consider that there is a single series of phenomenal differences between the experience of a white object in a good light and the experience of blackness; others consider that there are two series of phenomenal differences and distinguish between the series of increasing intensity and that of increasing whiteness[1]. Our experiments indicate that about one person in five differs from other people in the fact that there is only a single series of phenomenal differences in the hueless colour series—the whiteness of an object and its brightness are, to these persons, identical. It is difficult to convince such people that other subjects may see two papers as of equal brightness, but as differing in greyness or whiteness. Unless they have performed or seen such experiments as those described here, such persons, if they happen to be psychologists, can hardly fail to maintain a theory of the relationship between whiteness and brightness which is different from that required to account for the experiences of the majority.

V. RELATIONSHIP OF PHENOMENAL REGRESSION WITH OTHER FACTORS.

(*a*) *Intelligence.*

In the tables given above (Tables III, IV and V), it appeared that in all cases the correlation between I.Q. and test result was small and negative. The indication was that the more intelligent subjects tended to see more nearly to the stimulus character of the object, being less influenced by phenomenal regression to the 'real' character. Each individual correlation, however, was too small in comparison with its standard error to possess a sufficient degree of significance.

The fact that similar results are obtained for the three tests A, B and C somewhat increases the probability of a real relationship but to a small and not exactly calculable extent. Since the three tests are intercorrelated, their evidence of relationship is not independent, *i.e.* a chance of sampling that produced a spurious correlation of A with I would be likely also to produce a similar spurious correlation of B and C with I.

In order to discover whether phenomenal regression and intelligence are truly correlated, we may take as the best measure of P.R. the mean of the results of tests A and B. The correlation between this and I may be calculated for the combined group of men and women students. It is found for this whole group of 64 subjects that the correlation between

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[1] Köhler, for example, distinguishes between "Oberflächenfarben" and "Lebhaftigkeitsgraden" and says that of these only the first shows great resistance to change under changing illumination (5). Equating these terms with 'whiteness' and 'brightness' respectively, the above experiments confirm this statement as a general rule. There are, however, many individual exceptions. Brightness shows great resistance to change in some subjects, and whiteness shows little in others.

$(A + B)/2$ and I works out as $-0{\cdot}245$. The probability of this being due to a sampling error is about 0·05. The correlation is probably significant[1].

The second intelligence test O was administered to fewer subjects, but since it shows all round higher correlations than I, its results give a somewhat better measure of intelligence. The correlation between $(A + B)/2$ and test O for the 53 subjects who did this test (27 of group M and 26 of N) was $-0{\cdot}41$. The probability of this correlation occurring by the chances of sampling is less than 0·01, so it is clearly significant[2].

We can, therefore, say with a reasonable degree of confidence that the tendency to phenomenal regression is related to intelligence, the less intelligent subjects showing the greater tendency to perceive the 'real' characters of objects.

(b) *Sex.*

The differences in the mean values of the indices of regression for men and women students of the same average age and intelligence is shown in Table VIII. The differences are not large, but for all tests lie in the same direction. With the exception of B, all differences are significant. The column for $(A + B)/2$, which we may take as the crucial test for difference, shows a significant difference.

Confirmation by calculating χ^2 for a fourfold table of the men's and the women's scores in $(A + B)/2$ about the median of the group as a whole, yields (as is to be expected) a smaller degree of significance, but gives a value of χ^2 which is 'probably significant.'

We may, therefore, conclude with a sufficient degree of confidence that there is a real sex difference in the tendency to phenomenal regression, women tending to see slightly nearer to the real characters of objects than do men.

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[1] The requirement I am taking as a standard of significance is that the probability of the relationship observed being due to the chances of sampling is not less than 0·02, *i.e.* it would not occur by chance more than once in fifty times. A quantity is, therefore, regarded as significant if it is not less than 2½ times its S.E. (or 3½ times its P.E.). If this probability is greater than 0·05, the evidence for asserting the relationship is regarded as insufficient, *i.e.* the measurement is not significant. If the probability lies between 0·02 and 0·05, the evidence is less strong than we should like, but it is considerably more likely than not that the relationship indicated is a real one. We may, therefore, say that such figures are 'probably significant.'

[2] This and the preceding estimate of significance have not been made by the usual method of calculating the probable error of the coefficient by the formula $0{\cdot}6745 . (1 - r^2)/N^{\frac{1}{2}}$, since this is stated by Fisher to give an exaggerated estimate of significance. His estimate of probability of sampling error depends on the transformation of r into a normally distributed measure and the use of 'Student's' principle for small samples. The probabilities given above are from Fisher's Table V A (6).

Table VIII.

		Age		I.Q.		Test A		Test B		Test C		$(A+B)/2$	
Group	N	Mean	M.V.	Mean	S.D.	Mean	S.E.	Mean	S.E.	Mean	S.E.	Mean	S.E.
N	27	20·9	0·63	124·2	5·62	0·697	0·0209	0 801	0·0172	0·392	0·0299	0·749	0·0148
M	42	21·4	0·70	125·5	7·62	0·613	0·0238	0·753	0·0187	0·2925	0·0287	0·683	0·0197
Differences and S.E. of differences						+0·084	0·0317	+0·048	0·0254	+0·0995	0·0415	+0·066	0·0246
Probability of occurrence by chance						0·008		0·06		0·016		0·008	

Table IX.

	Above median	Below median
Women	17½	9½
Men	17	25

$\chi^2 = 3{\cdot}895$. Number of degrees of freedom = 1. Probability of chance occurrence = 0·05.

(c) *Temperament.*

Experiments with the first subjects seemed to indicate that those of introverted or 'schizothymic' temperament saw much more closely to the stimulus or perspective object than did the extraverted or 'cyclothymic.' It seemed even possible that this might be a measurable result of the greater interest in outside reality of the cyclothyme and in the subjective world of the schizothyme. Two groups of 14 each were chosen as representative of these two temperaments from the personal knowledge of the experimenter, using Kretschmer's description of the typical schizothyme and cyclothyme as basis (7). This method of selection was obviously open to criticism as too subjective. It might well obscure a real relationship which would be revealed by an objective method of temperament classification applied to a larger number of subjects.

The number of subjects compared is too small for reliable estimation of the significance of difference by the usual method for large groups. It is necessary, therefore, to use Fisher's modification for this problem (6) of 'Student's' method of estimating the significance of a measurement derived from a small sample (8). This involves the calculation of a quantity 't' of which the probability of the occurrence by chances of sampling of different values for given values of $(N_1 + N_2 - 2)$ are given in Fisher's Table IV (6). A difference was found in all three tests, the cyclothymic group showing the larger mean index of regression. To save space, results of $(A + B)/2$ only are shown in Table X (top). The difference is small and is not significant.

It was thought that such difference as is found might be due to the fact that the mean age of the cyclothyme group is greater than that of the schizothyme, since phenomenal regression increases with increasing age[1]. This disparity in age is probably due to the fact that the marks given by Kretschmer for the cyclothymic temperament become more noticeable as middle age advances. Two comparable age groups were therefore obtained by dropping the extreme ages in both groups. The results are shown at the bottom of Table X. There is still a difference which is significant.

Table X.

			Mean of tests *A* and *B*		
		Mean age	Mean		Σ dev.^{2}
Cyclothymes	14	43·4	0·773		0·1824
Schizothymes	14	35·75	0·701		0·1943
Difference and *s* of difference			0·072		0·120
t				1·59	
Probability of occurrence by chance ...				0·1–0·2	
Cyclothymes	10	39·25	0·780		0·1148
Schizothymes	8	39·0	0·710		0·0730
Difference and *s* of difference			0 070		0·108
t				1·37	
Probability of occurrence by chance ...				0·2	

Since this difference is not greater than would occur once in four times between random samples drawn from identical populations, it gives no reason for concluding that there is any relationship between phenomenal regression and temperament. More satisfactory evidence might be drawn from a larger group and with better methods of distinguishing temperaments.

(*d*) *Age.*

Group *O*, of age range 24–59·5 with mean at 37·7, shows a larger index of regression for all tests than group *M* of mean age 21·25. The differences are not, however, statistically significant. A clearer indication of the probable dependence of phenomenal regression on age is obtained by splitting group *O* into four age groups of equal range. The result is shown in Table XI. It is seen that there is a general tendency for the mean indices to increase with age.

In order to determine the significance of this apparent increase, the mean of tests *A* and *B* was again taken as the best measure of the P.R. tendency. Since there is no reason why the ages of subjects should show any approximation to normal distribution, a correlation coefficient would be an inappropriate measure of the required relationship. The regression of $(A + B)/2$ on age was therefore worked out for the whole 36 cases and the equation found was $(A + B)/2 = 0{\cdot}572 + 0{\cdot}00425 \times$ years of age.

In order to determine whether there is a real increase of P.R. with age, it is necessary to determine the significance of the coefficient 0·00425.

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[1] Shown in next sub-section.

The standard error of this was found to be 0·002, making the probability of its occurrence by the chances of sampling about 0·03. This figure is, therefore, probably significant.

The low significance of this figure is partly the result of the heterogeneity of this group which increases the scatter of individual values from the regression line. This group was not, in fact, arranged with the intention of investigating the relationship of P.R. with age, since the existence of a relationship was not suspected until the series of experiments was nearly completed. More satisfactory evidence could be obtained from a group as homogeneous as possible with respect to intelligence and occupation.

Table XI.

Age group	N	Mean score. Score in $(A+B)/2$
Below 28 5	10	0·6795
28·5–38·5	10	0·7035
38·5–48·5	11	0·780
Over 48 5	5	0·786

From these results, we can say only that the tendency to see objects in their 'real' characters probably increases with increasing age. As additional support we have Beyrl's work with children (9) who, working with the size experiment alone, found that the tendency to 'constancy of size' increased from two to ten years, and was still greater with an adult group.

VI. Dependence of phenomenal regression on training in art.

The painter, etcher or drawer is trained to reproduce the stimulus or 'perspective' shapes and sizes of objects which he discovers by such devices as closing one eye and making measurements with a pencil held at arm's length at right angles to the line of vision. It is possible then that, either as a result of his training, or as a result of his continual practice, the tendency to phenomenal regression to the 'real' character of the object either disappears altogether or is reduced in amount.

In order to discover whether this was the case, a group (P) of 12 male art students of the same age range as the group of university students and a group (Q) of 10 male artists and teachers of art of the same age range as group O were given tests A and B[1].

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[1] C was discarded in order to save time in the testing, since it had been found that the index of regression for brightness was an unsatisfactory measure, and I did not then know that satisfactory results could be obtained from C if the index of regression for whiteness were substituted for that of brightness.

That the tendency to phenomenal regression does not disappear altogether by practice in drawing is shown by the fact that there was not a single one of these subjects who did not show a positive index of regression in both tests.

In order to discover whether there is a significant difference between the groups trained and practised in art and the control groups, it is again necessary to use the method for small samples.

The results are shown in Table XII. The last column but one gives the values obtained in the experiment preliminary to *A* in which the subject reported when an inclined elongated ellipse was phenomenally equivalent to a circle. This was included because drawing a circle is a much more familiar operation than reproducing an inclined ellipse. So far, therefore, as the effect of art training is specific to the actual operation performed rather than a general lowering of the tendency to phenomenal regression, the reduction of the index for the artist group should be less for this less accustomed observation. It will be seen that there is a considerable indication that it is less, suggesting that the lowering of phenomenal regression is partly specific to the operations ordinarily performed and partly general.

Table XII.

Group	No.	Mean age	M.V. of ages	Test *A* (inclined circle)		Test *B* (sizes)		*A* preliminary (inclined ellipse)		$(A+B)/2$	
				Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
P (art students)	12	21·4	0·9	0·531	0·186	0·679	0·154	0·632	0·187	0·605	0·151
M (university students)	42	21·4	0·9	0·613	0·155	0·753	0·121	0·708	0·121	0·683	0·128
Difference of means and *s* of difference				0·082	0·162	0·074	0·129	0·076	0·137	0·078	0·133
t...				1·55		1·75		1·70		1·78	
Probability of difference being due to chances of sampling				0·1–0·2		0·05–0·1		0·05–0·1		0·05–0·1	
Q (artists)	10	44·5	9·75	0·448	0·139	0·631	0·149	0·605	0·152	0·540	0·088
O (control group)	36	37·7	9·3	0·671	0·159	0·792	0·130	0·748	0·127	0·732	0·134
Difference of means and *s* of difference				0·223	0·155	0·161	0·134	0·143	0·132	0·192	0·126
t...				4·03		3·36		3·035		4·27	
Probability of difference deing due to chance				<0·01*		<0·01		<0·01		<0·01	

* Fisher's tables of *t* do not give values of *P* below 0·01, since such values indicate significance and their precise amounts is of no practical importance. Approximate values can be obtained, since *n* is large (*i.e.* 44) by using normal probability tables. This gives, for this line, values of 0·0006, 0·0008, 0·0025 and 0·00002.

We can check these estimates of significance by a method not assuming normal distribution of the measurements. If the results of the art students and the university students are considered together, then one-half of each class should be above and one-half below the median for the whole group. The significance of any observed deviation from equal division by the median of the whole group can be determined by the estimation of χ^2 by the ordinary method for fourfold tables with one degree of freedom (6). The result for the mean of the tests A and B is shown in Table XIII. It will be seen that whereas there are only two more art students below the median than above (which might happen by chance as often as not); in the artist group there is not a single individual who is not less than the median of the whole group, which would happen by chance only once in 2500 times.

Table XIII. *Estimation by χ^2 method of the significance of the difference (top) between art students' scores in $(A+B)/2$ and control group, and (bottom) artists' scores and control group.*

	Number below median	Number above median
Art students	7	5
University students	21	23

$\chi^2 = 0{\cdot}43$. Probability of chance occurrence $= 0{\cdot}5$.

	Number below median	Number above median
Artists	10	0
Control group	13	23

$\chi^2 = 12{\cdot}78$. Probability of chance occurrence $= 0{\cdot}0004$.

A further question which was not investigated was whether other occupations produce an effect in the opposite direction. The work of the artist demands that he shall react as much as possible to the stimulus characters of objects. The engineering mechanic, on the other hand, must react to 'real' characters. For fitting a nut to a bolt, objects of the same 'real' size must be reacted to as equivalent; it would seriously interfere with his efficiency if they only appeared equal when they were the same distance from his eyes. It appears likely, therefore, that his practical training will tend to increase his tendency to phenomenal regression. This would be an interesting subject for investigation, but I have no data on the subject.

VII. SUMMARY.

The tendency to phenomenal regression to the 'real' character of an object is present in all persons. This tendency with respect to shape, size and 'whiteness' are shown to depend on the same group factor which may be called P.R. For most subjects (but not all) the immediate experience of brightness is different from the immediate experience of whiteness, and when there is this difference, a subject's probable point of balance for equal apparent brightness of two papers of different reflectivity differently illuminated shows little relationship with his phenomenal regression for size and shape. The degree of a subject's tendency to see the real characters of objects also depends on intelligence, sex, training in art and (probably) on his age.

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SENTIMENT AND SOCIAL ORGANIZATION[1].

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I. *Difficulties in understanding social behaviour assuming ego and sex instincts only* (p. 242).
II. *Trotter's assumption of a herd instinct* (pp. 242–243).
III. *Examination of this assumption* (pp. 243–245).
IV. *Freud's 'libido theory' of the group* (pp. 245–246).
V. *This theory inadequate* (pp. 246–247).
VI. *The sentiment-formation tendency* (pp. 247–249).
VII. *Summary* (p. 249).

I. DIFFICULTIES IN UNDERSTANDING SOCIAL BEHAVIOUR ASSUMING EGO AND SEX INSTINCTS ONLY.

IN any systematic endeavour to understand, to explain, and to foretell the behaviour of an individual in any given situation, certain fundamental assumptions are made, and, so long as they are useful, they are retained. Such assumptions are, for example, that each individual is endowed with tendencies which urge him to preserve his own life, and with others which urge him to preserve the life of his species. These are the so-called ego and sex instincts.

With these assumptions we get a certain distance in our study of the individual; but there are some facts which are, apparently, difficult to explain on these assumptions alone: some animals live in groups, while others are solitary; some may be altruistic, while others are utterly selfish.

II. TROTTER'S ASSUMPTION OF A HERD INSTINCT.

It was therefore suggested by Trotter, in articles published in 1908 and 1909, that a further assumption should be made: viz. that some animals are endowed with a 'herd' instinct while others are not. He elaborated this hypothesis in his book *The Instincts of the Herd in Peace and War*, which was published in 1916. He there remarks, p. 17, on the

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[1] Read before the Psychology Section of the British Association in London, September 1931.

'simple' behaviour of the tiger and the cat, which animals are wholly governed by the instincts of sex, self-preservation and nutrition, and the complicated behaviour "of the dog, with his conscience, his humour, his terror of loneliness, his capacity for devotion to a brutal master...which no sophistry can assimilate without the aid of a fourth instinct."

With the help of this assumption he proceeds to show how the complicated behaviour of the individual in the group can be explained.

The chief characteristics of Trotter's gregarious or herd instinct are:

(*a*) that the animal must live with the herd;

(*b*) that the animal is peculiarly sensitive to the voice of the herd; and

(*c*) that the animal instinctively obeys the dictates of the herd.

III. EXAMINATION OF THIS ASSUMPTION.

The first characteristic, that the animal must live with the herd, is a *sine qua non* of a herd instinct. Trotter, p. 31, says that an animal endowed with the herd instinct "will feel an unanalysable primary sense of comfort in the actual presence of his fellows, and a similar sense of discomfort in their absence....Loneliness will be a real terror, insurmountable by reason."

It is not necessary to elaborate the evidence for the fact that most animals exhibit all the signs of distress when away from the herd from which they may have been by any chance separated. Yet there is much evidence that does not quite fit in. In the first place herd animals, brought up away from the herd, do not willingly join the herd when given the opportunity to do so, nor are they immediately accepted by the herd should they endeavour to join it. The dog, brought up away from other dogs, remains with his master. He may be attracted from his home for a short space of time by another dog of the opposite sex, or he may go off to chase a cat or a rabbit, but he speedily returns to his master's home, which is, for him, his herd. Similarly, if his master moves to another town, the dog will exhibit all the signs of hesitancy in meeting any dog of his new neighbourhood, and he will, very probably, be set upon by the local dogs and have to fight for his footing before he is accepted. Again a lamb, brought up in a house, follows its mistress about 'like a dog' when it grows older, and bleats piteously when she is absent. When returned eventually to the flock it shows none of that "unanalysable primary sense of comfort in the actual presence of its fellows," which it ought to show according to Trotter, but on the contrary exhibits a great "sense of discomfort" in the absence of its mistress.

It is obvious, then, that this 'herd instinct' is something different from what is usually considered instinctive. It does not appear necessary for the 'herd' animal to be with animals of his own species at all. In fact he may avoid them.

Again, if it be instinctive for one species of animal to herd with its own kind and to exhibit signs of distress when separated from them, while another species of animal has no such instinctive urges, it would seem that the latter species would be more amenable to confinement and would make the more satisfactory pets. Their comparatively few instinctive wants would be easily provided for, and they would not suffer from the deprivation of the satisfaction of a herd instinct, since they have no such instinct. This deduction is notoriously incorrect. It is the herd animal which takes most kindly to confinement and to domestication, not the solitary animal.

The second and third characteristics of the herd instinct, viz. the suggestibility of the individual animal to the opinion of the herd, and the instinctive necessity to obey the herd demands, are postulated by Trotter to explain the facts of group behaviour.

Suggestibility there must be. Without it group life would not be possible. Yet it can easily be seen that it is not necessary to postulate a herd instinct to explain the phenomenon. It is only necessary to postulate ability to interpret signs, received through the sense organs, however unwittingly, to explain suggestibility.

But the "instinctive obedience" to the herd order to do a certain deed "which may actually be unpleasant," requires a little more consideration.

I find Trotter rather obscure on this point. On p. 48 he speaks of the herd instinct as exercising "a controlling power upon the animal *from without*." (My italics.) "With the social animal controlled by herd instinct," he continues, "it is not the actual deed which is instinctively done, but the order to do it which is instinctively obeyed. The deed, being ordained from without, may actually be unpleasant, and so be resisted from the individual side and yet be forced instinctively into execution." Later, on pp. 82 and 83, perhaps as a concession to Freud, for this part of the work was written after the earlier part, Trotter appears to change his ground somewhat, for he says that "the repressing forces (*i.e.* the herd demands) become actually constituent in the child's personality, and as much part of his being as the egoistic desires with which they are now able to contend on equal terms." At this stage Trotter clearly felt that mental conflict would not arise if the herd behests were 'ordained from without.' This is to a certain extent true. If an individual is held up by external forces, or is compelled by external circumstances to do something he does not wish to do, the mental consequences are not so serious as they may be if the struggle is in himself, between his impulses and his sentiments, for example. The idea that "the repressing forces become actually constituent in the child's personality, etc." is very similar to Freud's view of repression as being due to the 'ego-ideal,' a development of the 'ego,' though the actual mechanism is slightly different.

The point Trotter makes then is that the individual instinctively incorporates in his own personality the herd opinions and dictates. If, for the sake of the argument, we accede this view, it would seem of necessity to follow, that the earlier and the more insistently the herd standards and views are forced on to the young child the better. He must be given no time in which to form socially disapproved habits or opinions. The herd standards must be rigorously imposed from the beginning. The falsity of this deduction is obvious. The child who has been disciplined and educated in this way is almost invariably the one who departs most strongly from the precepts he has received when the opportunity occurs. The 'well brought up' child, in this sense, is an intolerable nuisance. He is, as a rule, selfish, self-centred, and cruel. He takes a keen delight in bullying those younger than himself, and usually appears to be quite devoid of all social qualities. On the other hand, the child who is universally liked, and whom everyone welcomes, is almost always the one who has not had the herd standards and demands 'forced' upon him.

It is, of course, possible to explain this contrariness by saying the forcing has been overdone. This may be true; but then that would mean that the 'instinctive acceptance' has not taken place.

It appears, then, that the assumption of the herd instinct does not fit the facts. If that is so, it would seem that we must begin again our search for the reasons of the differences in behaviour between the herd and the solitary animal.

IV. FREUD'S 'LIBIDO THEORY' OF THE GROUP.

Freud disagrees with Trotter's views, but not on the grounds we have been considering. He criticizes Trotter's approach to the problem because to Trotter the leader of the group or herd is of minor importance, whereas Freud maintains that "it is impossible to grasp the nature of the group if the leader is disregarded" (*Group Psychology and the Analysis of the Ego*, p. 88).

Freud goes back for his origins to the "primal horde ruled over despotically by a powerful male" (p. 90), and proceeds to show how the sons, being denied mates, eventually banded themselves together and killed their father. "He (the father) forced them," Freud says on p. 94, "into abstinence and consequently into the emotional ties with him and with one another which could arise out of those of their tendencies that were inhibited in their sexual aim. He forced them...into group psychology. His sexual jealousy and intolerance became in the last resort the causes of group psychology."

The origin then of all groups is in the banding together of the brothers which occurred, we are to understand, in the primal horde because the brothers were denied mates.

The way in which social feeling develops is seen, according to Freud, in what may happen when a number of individuals fall in love with the same person. When this occurs jealousy may arise between the rivals, but if the beloved object is unattainable the individuals will be bound together, a communal or group feeling will develop. "We have only to think," Freud says on p. 87 of *Group Psychology and the Analysis of the Ego*, "of the troop of women and girls, all of them in love in an enthusiastically sentimental way, who crowd round a singer or pianist after his performance. It would certainly be easy for each of them to be jealous of the rest; but, in face of their numbers and the consequent impossibility of their reaching the aim of their love, they renounce it, and, instead of pulling out one another's hair, they act as a united group, do homage to the hero of the occasion with their common actions, and would probably be glad to have a share of his flowing locks. Originally rivals, they have succeeded in identifying themselves with one another by means of a similar love for the same object." "Thus social feeling is based," Freud adds on p. 88, "upon the reversal of what was first a hostile feeling into a positively toned tie of the nature of an identification.... All the members must be equal to one another, but they all want to be ruled by one person. ...Man is a horde animal, an individual creature in a horde led by a chief."

The "higher, moral, spiritual side of human nature," which Trotter explains as being due to the submissiveness of the self to the herd dictates, Freud says are "in the ego-ideal or super-ego, the representation of our relation to our parents" (*The Ego and the Id*, p. 47), the ego-ideal being formed, as a development of the ego, owing to the Oedipus complex.

V. THIS THEORY INADEQUATE.

Freud's analysis is interesting, but it is not at all convincing. It is very possible that the band of brothers might be formed as he suggests, and that the troop of women and girls might form a social group, but the generalization which demands a leader as the primary cause is not wide enough. It is not the leader, but the common interest, which brings the individuals together. It is not the leader of a pack of wolves which forms the pack, it is the object in which each is interested, the prey. If food is plentiful the pack breaks up. And it is the same with our human societies and groups.[1]

Groups are formed round common interests, and when the group has been formed the individuals may form sentiments, as we shall see later, round the group itself, so cementing the group together and preventing its disintegration when the primary cause of the collecting together no longer exists. This point was developed by McDougall in his *The Group Mind* in 1920, and has been elaborated further, more recently, by Bartlett, in his *Psychology and Primitive Culture* and in *Psychology and the Soldier*.

Again, Freud's analysis in no way enables us to understand why it is that some species of animals form herds while others do not, and his explanation of the changing "of what belongs to the lowest depths in the minds of each of us...into what we value as the highest in the human soul" through the formation of the ego-ideal (*The Ego and the Id*, p. 48) seems rather far-fetched, but that, of course, is no reason why it should not be possible.

VI. THE SENTIMENT-FORMATION TENDENCY.

I wonder whether in these analytical attempts to find the germinal springs, in the continual endeavours to arrive at the origins, we are not tending to overlook and to forget that the mind is an organism, and that there is inherent in the mind the very property of higher and still higher organization. The analytical method is very valuable; but we must hesitate before we accept the view that what is found on analysis is there in its elemental form in the intact body or mind. A molecule of water is something different from two atoms of hydrogen and one of oxygen: a sentiment is more than a mere collection of emotions and of tendencies to action and so on. In fact the expression and character of the elements may be modified beyond recognition when the new organization has been formed.

This is where I feel that Trotter and Freud have gone astray. They have overlooked the fact of the innateness of the tendency to organization. There is nothing new in the idea of organization; it was emphasized by Shand in his work *The Foundations of Character* in 1914. The law of organization was his first law of character.

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[1] Rivers distinguishes between human groups which have leaders and others which have none. He, however, explained group formation as being due to a 'herd instinct.' *Instinct and the Unconscious*, p. 95; *Psychology and Politics*, p. 40.

The point is that there is an innate 'sentiment-formation tendency' in man and in many other animals. This is the main difference psychologically between the dog and the cat. The possibility of the organization of the emotions and instincts to form sentiments is apparently very small in the cat and is large in the dog. The dog is endowed with the capacity for the organization of his innate tendencies round animate objects. Man has this tendency also. But man is also endowed with the ability to form sentiments round ideas and ideals—whether animals can think and form ideals and so form sentiments around them I do not know. When sentiments have been formed round ideals the most altruistic of behaviour may result.

That sentiment formation is not due to repression[1] is obvious from the difference in the behaviour of a cat and a dog brought up in the same environment. Sentiment formation also occurs naturally, as far as one can judge, in human babes brought up away from other human beings. I refer to such cases as the wolf children of India, where the children obviously develop sentiments round the family circle into which they have been admitted.

The fact that some animals are herd animals while others are not is explained by assuming that the one class of animal is endowed with the sentiment-formation tendency while the other is not. Animals of the first class, when brought together, form sentiments round each other; those of the other class do not. In nature the young are reared with their own species round them so that, the opportunity being given, animals of the one class form sentiments round members of their own species, and a herd is formed. If they are reared by others than their own parents they are almost as devoted to their foster parents as they would be to their own kind. If there is no sentiment-formation tendency, no herd is formed. It is not without significance that all our domestic animals, with the exception of the cat, are herd animals—and the cat is obviously very different from the other animals.

The paradoxical (from the herd instinct point of view) behaviour of children is easily explicable. If the child has formed sentiments of one kind round his nurse or round his parents he will be an angel when out at parties. His behaviour is dominated by his love for his nurse or his parents, and in consequence, and without any effort, he behaves as he knows they would have him behave. If his sentiments are of another kind his behaviour will be the opposite of angelic. Those nurses who endeavour to enforce herd standards, who rely on repressions, are preventing the formation and development of the social sentiments which they are endeavouring to implant. The normal line of development starts in the individual's family and in his early surroundings and widens gradually. Eventually many sentiments of divers sorts may be formed, and so his behaviour will vary according as one sentiment or another is dominant: at one time he will be sociably inclined, at another solitary.

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[1] I think a good case might be made out for the thesis that sentiment formation is never due to repression, though repression may be a factor determining the character of the sentiment when formed. On the other hand, sentiments are often very efficacious repressing agents.

VII. SUMMARY.

It appears that the attempt to find the foundation of group behaviour in instinctive tendencies fails, as does the psycho-analytic 'libido' theory. The reason is that the group arises on account of the organizing tendency there is in the mind, and analysis, while finding the mental elements, breaks down, in the process, that for which it is seeking.

Animals which have the capacity for organization—the sentiment-formation tendency—form more or less permanent sentiments round suitable objects. These have a very determining influence on their behaviour.

If such animals come together, either fortuitously or through some common interest, they are very liable to form sentiments round each other, and a 'herd' or group will be the outcome. The 'herd' may just as well consist of a boy, a dog, and a lamb as of animals of the same species.

When sentiments are formed round ideals altruistic behaviour is possible and probable.

Finally, one of the most pressing of psychological problems is the study of the conditions which are most conducive to the formation of sentiments and the application of such knowledge to the world problems of to-day. For these problems are largely due to the existence of sentiments which have lagged far behind in their progress the progress in other fields. Such lack of balance is fraught with the gravest dangers.

THE INCEST BARRIER: ITS RÔLE IN SOCIAL ORGANIZATION.

BY BRENDA Z. SELIGMAN.

I. *The problem*

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I. THE PROBLEM.

(*a*) *Introduction.*

CIVILIZATION may be regarded as the measure of the capacity of man to live in organized groups; to this end natural impulses become modified for social gains. One problem that faces the anthropologist is to ascertain the nature of the fundamental human group from which has sprung the various civilizations of the world, both rude and complex, as we know them in history and to-day. This is no academic problem, but one of vital importance, for if after examining all the known cultures of the world we are able to find some fundamental similarity in structure, we may hope to arrive at conclusions regarding the function and value of this structure in human society. At the present time, when modern inventions are speeding up the interaction of cultures at an unprecedented rate and hence causing the breakdown of old types and the formation of new ones, the question is not only one of responsibility towards the backward races whose *status quo* we are disturbing, but it is also important for us to ask ourselves what is the basis of our civilization; what can we afford to jettison, and what should we retain even though the present form may require modification?

The qualities in man which condition the human group in distinction to other animal groups are:

(1) The absence of a breeding season[1].

(2) The long immaturity of the young, with the development of sexual potency before real physical or mental maturity is reached.

(3) Speech and the communicability of ideas, *i.e.* the possibility of cultural development, borrowing and change.

(4) The growing command over nature, and hence over the food supply.

These conditions both demand and make possible a type of social organization that is essentially human. What then is the social group in which non-seasonal sexual needs can be gratified, the young can be protected during infancy and adolescence, culture can be carried on from generation to generation, and co-operation secured in procuring food? To "the man in the street" the answer is perfectly clear. He knows quite well that his infancy and childhood were protected in the family; in the family he learnt his speech and gathered those ideas which seem a 'natural' element in his culture; outwardly, at least, he accepted the family sex taboos, and when as he grew up other social groups opened out to him he looked to the family still for food and shelter. Even if he revolts from the group in which he was brought up, he desires consciously or unconsciously to form another similar group.

Why then does this truism require demonstration to the social psychologist? Partially perhaps because of repression, but mainly because a whole body of facts observed among the lower cultures seem to point in an opposite direction. Chief among these are the classificatory system of kinship, exogamy, the levirate and sororate, and various complicated marriage prohibitions which permit marriage only between persons born into certain groups. Also, periods when all prohibitions are removed and licence permitted, or even ceremonially enforced, have given a fictitious probability to the theories of primitive promiscuity, group marriage, or sexual communism.

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[1] It has recently been suggested that this first characteristic is shared by most if not all other primates, but observation in a wild state is peculiarly difficult. The late S. S. Buckman, writing as James Corin (*Mating, Marriage and Status of Women*, 1910), directed attention to the importance in man of the capacity to rape, and this has recently been reaffirmed by Dr Gerrit S. Miller, who considers that the behaviour of man is differentiated from that of the anthropoids by a "socially effective sentiment of love, and the physical and psychological possibility of rape" (*Journal of Mammalogy*, 1928).

I cannot here examine these theories, I will only point out that they all neglected the bonds by which social cohesion came about. It had to be presumed that authority rested on pure force, modified by some such factor as 'herd instinct.' Now it is clear that the continual exercise of force could allow only a numerically weak group to form, and that the beginning of herd instinct can only arise in the group in which the animal is born, so that even should we regard herd instinct as attaining special vigour in human society it could only arise in the family.

It is true that there have always been some supporters of the family as the primitive human group, notably Westermarck, and, from his earliest writings, Malinowski. Westermarck, however, founded his contention on a theory of incest which presents very great difficulties, while the evidence that can be adduced from anthropology as well as that brought forward by psycho-analysts and social workers does not uphold his argument; even were it granted that cultural influences so early affected the sentiments of young people brought up together, both in the life history of the individual and in the history of the race, as to cause the natural aversion to sexual intercourse between housemates postulated by Westermarck, it cannot be shown how such supposed aversion leads to stringent laws of marriage prohibition, which are widespread in savage cultures and are best exemplified by exogamy.

I must acknowledge my debt to Malinowski, who has laid stress on the sociological necessity of the incest barrier and shown possible lines of development of the clan from the family, although I have been unable entirely to follow his treatment of the incest prohibition of the brother-sister type, or of descent in the family.

Having for many years studied the complexities of the classificatory system, I have naturally been impressed by the importance among the ruder cultures of that larger group of persons, only distantly related to the individual, comprising not only the clan[1] but also the clan of the *other* parent (to whom clan relationship is not traced) and the clan of the spouse. In most savage societies the behaviour towards all persons within these larger groups is regulated by custom, being based on the behaviour pattern and the terms of address used towards immediate relatives (the parents, the brothers and sisters, the spouse, and the parents of the spouse). The wide distribution of this type of organization, and the contrast which it appears to form to the European family, led many anthropologists to neglect the smaller family group, which is always to be found working within the clan organization.

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[1] In *Notes and Queries in Anthropology* (5th ed. 1929, p. 74) a clan is defined as "an exogamous group in which descent is reckoned on a unilateral basis, either matrilineal or patrilineal."

When Freud discovered a technique for investigation of the unconscious, and the findings of psycho-analysis became known to sociologists, it seemed that new light should be thrown on sociological studies. The unconscious reactions of adult civilized folk, as well as much of the still conscious behaviour of children, showed remarkable similarity to the rites and customs of savages. The 'omnipotence of thought,' and the place occupied by animals in the unconscious, are facts of the greatest value in understanding magic and totemism. But the greatest gain of all was the discovery of the ever present incest conflict within the family group of the civilized. As a nuclear complex—the legacy of a bygone drama enacted in the dim past[1]—it has little interest, but as an ever present problem for which some solution must be reached in every type of culture the idea of persistent incest conflict is most stimulating. We are thus directly indebted to the psycho-analysts for demonstrating that the behaviour towards near relatives is one that every individual has to learn during infancy and childhood, that it is compounded of conflicting emotions of love, hate, and revolt against power, and that successful solution makes the individual a normal member of society—failure, an outcast either emotionally or morally. If this is so in Western European civilization, with its rich social heritage, its religious and legal sanctions, it is small wonder that savage man goes to great lengths in order to solve the problems that are involved.

What is the bearing of the incest conflict on our understanding of primitive man and the fundamental human group? If we agree with Malinowski that in the modification of instinctive impulses and the establishment of a moral law (viz. the incest prohibitions) the germs of ethics and culture were born, then we must suppose that primitive man developed in a group in which this regulation was essential. Such a group is the human family. When we have examined the reasons for accepting the family as the fundamental human group, and suggested the sanction that holds it together, we have still to ask what influence the family has on the varied and complicated marriage regulations throughout the world.

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[1] Sigmund Freud, *Totem and Taboo,* New York, 1918

(*b*) *Freud's contribution to the problem.*

It is unfortunate that, having demonstrated the conflict in the family, Freud turned away from his own teaching and revived the animal 'horde' of *Homo alatus* put forward by Atkinson[1]. This horde was supposed to be composed of one adult male, still a speechless creature, and as many females as he was able to keep—thus mating with his own daughters as well as his originally captured wives. The young males were kept in subjugation or driven out, and family affection was unknown. Then it was that, according to Freud, the Œdipus drama was enacted and the 'brother clan' was formed. But he does not describe the sociology of the 'brother clan'; presumably he does not follow Atkinson further, who supposed that it was the force of maternal feeling that kept at first the youngest and then other sons in the family group. "Society," Freud says, "is now based on complicity in the common crime, religion on the sense of guilt and the consequent remorse, while morality is based partly on the necessities of society and partly on the expiation which this sense of guilt demands[2]." Thus he seems to support the idea that totemism and exogamy arose in the 'brother clan,' and that in such a group the basis of human society was formed[3].

If we return to the theory that it is in the family itself, not in some larger group governed by exogamy, that the foundations of culture were laid, we have behind us all the findings of psycho-analysis as well as those of social anthropology, for, as Malinowski has pointed out, no society exists, however complicated its social grouping, in which the family is not also present.

Just as the psychologist sees the individual make-up profoundly influenced by the first few years of life, so it is reasonable for the social psychologist to deduce the corollary *autres temps autres mœurs*—other conditions, other solutions to the eternal problem. The universal nucleus must, however, be recognized: the end in view is the formation of a harmonious group in which it is possible to retain the physically and mentally immature adolescents (who are, however, sexually potent). I do not suggest that this was a conscious aim, but the advantage gained by those groups that achieved this difficult result was so great that they have been able to leave offspring to carry on their traditions and so have come to inherit the earth.

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[1] *Primal Law*, 1903.

[2] *Totem and Taboo*, p. 241.

[3] Even this renunciation of the mother and banding together of brothers to form a group gives no hint of the origin or function of the brother-sister incest prohibition.

What then are the constant factors in the human family group, and what are those imposed by culture? At what age will any of these factors affect the child? If theory can be a guide to research in the discovery of new facts, these points are important.

Monogamy and polygamy, the type of descent and of inheritance, infanticide or its absence, the practice of infant betrothal, pre-nuptial freedom, or enforced continence for one or either sex, must one and all affect the child in the family group and will react on the sexual life of the individual. But the period when these factors begin to influence the child should be studied, in order to observe whether they affect the primary attachments formed during infancy or later developments in childhood or adolescence. The problem for the social psychologist is thus twofold. He must define the composition of the fundamental human group, and trace the relationship to this group of the other social groups found at present among the lower cultures. He must then examine those customs and behaviour patterns among the ruder cultures, which apparently help to enforce the regulations of society and to uphold its cohesion, and ask how these reactions correlate with the fundamental family conflict. It will be found that some societies stress one aspect of the problem and others stress another, but in none does the emphasis fall exactly where it might have been expected according to psycho-analytic theory.

II. THE FAMILY GROUP.

The first part of the problem must be passed over briefly: much has been written already, and it is unnecessary here to make a comparative survey of the world to show that the family group founded on individual marriage is found everywhere; it is easier to say that there is no known society where its absence has been attested. But the family group founded on individual marriage implies neither monogamy nor lifelong partnership; all that is meant is that the mating of any pair of persons is socially recognized, and this recognition carries with it social obligations.

III. THE CLASSIFICATORY SYSTEM.

I will deal with the classificatory system very briefly. This system is closely associated with the clan organization; it has certain well-recognized forms and many important modifications. It is found almost all over the world, in Asia, Oceania, Africa, and America; traces of its existence have been seen by some scholars in Europe, and are also present in China. The outstanding feature, in contradistinction to our own family system, is the extension of the use of terms denoting physiological kinship to whole classes of persons, to some of whom physiological kinship cannot be traced. These persons are related to the individual either by real or fictitious links with persons who themselves are related. Examples may be taken of the simplest classificatory relationships, mother-child and brother-brother (or brother-sister). A child uses the term 'mother' not only to his own mother but to his mother's sisters, not only to the true sisters of the mother but to those whom she calls 'sister.' All these women call him 'child.' He calls all the other wives of his father 'mother,' and as (by the same law which extended the use of the term 'mother' to his mother's sisters), he calls his father's brothers 'father,' so he calls the wives of his father's brothers 'mother.' As the mother's sister is 'mother' so the mother's sister's child is 'sister.' We must now return to those 'mothers' who are 'mothers' because the true mother addresses them as 'sister,' and we shall see that the children of such women are again 'brothers' and 'sisters.' Thus a vast number of persons of the correct generations become either 'brothers' or 'sisters' to one another, or 'parents' and 'children.' Yet the recognition of this classificatory relationship between whole groups of persons never obliterates the closer family ties which exist at the same time.

With a system so widespread it is not surprising that the theory arose of a group larger than the family as the primitive group, with the family as an evolution from this larger group[1].

The classificatory system of kinship is usually associated with the clan, and clan organization is of great social value to those who possess it; it makes possible undertakings that would be beyond the scope of the family group, it fosters tradition and customs, and it gives to every member a social status. In carrying over the relationship terms from the family to the clan the gain in stability is very great, for these terms are already associated with certain rules of life which rest on sanctions of the most intimate kind: sanctions moulded by tradition, not imposed from outside but formed afresh by each individual as he reacts from infancy onwards to his emotional and social environment. It is clear that if submission to the incest prohibitions within the small family group is successful an harmonious group is produced, a group in which the adolescents in exchange for their freedom of action gain the protection of the older generation, while the older generation in exchange for the limitation of sexual liberty gains the co-operation of the younger generation. To produce this harmony, fear of the powers of the elder generation must be modified to awe and veneration.

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[1] I have already mentioned the chief evidence that leads me now to reject this theory and to put in its place the evolution of the classificatory system and the clan from the primitive family, and will only refer here to the works of Malinowski and to my paper, "Incest and descent, their influence on social organization," in *J. Roy. Anthrop. Inst.* 1929, LIX. For an account of the classical controversy see Westermarck, *History of Human Marriage*.

The process by which this comes about is twofold. The elders gain prestige for their knowledge of traditional lore and their skill in magical practice, but still more from the fact that in savage society a man's influence rarely ceases at death. A dead father or ancestor is still interested in the welfare of his offspring, and punishes them for neglect of himself or for other misdeeds; prosperity can only be hoped for when the ancestors are pleased. Hence transgression of the incest law offends not only the living parents but the dead ancestors. The sanction for the incest law is therefore religious; breaches are seldom punished by society, but punishment is usually automatic, and the evildoers either die or suffer from disease or barrenness.

If we may suppose that the boundaries of the family kinship group were extended until the clan was formed with the aid of the extended use of the kinship terms, we should then expect just what we find so commonly in savage society: clan brotherhood and the separation of society into generations, and ancestor worship merging with the cult of heroes, a basis favourable to cultural growth and development.

IV. MARRIAGE.

The type of marriage consistent with primitive society must be examined before we can consider the extension of the incest laws to wider marriage prohibitions. The type of marriage depends ultimately on economic conditions, for though monogamy exists especially among hunting peoples there is no need to suppose that the monogamous marriage is necessarily the primitive type from which all others have evolved. Indeed there is a factor in favour of polygamy which may have a biological basis[1]. Among many savage peoples coitus is forbidden to women during the later months of gestation and for the period of lactation. This regulation, especially during lactation, is accepted by women quite readily. It may be that lactation and the care of an infant does satisfy all a woman's sensual needs; the custom certainly has survival value, for a child born to one woman every few years instead of yearly has a greater chance of care than one that is deprived of the breast after a year. If the economic conditions are good enough for a man to take more than one wife there is a greater chance of infant survival, and, owing to the interest in the child, sexual jealousy is not necessary between co-wives, given that the conditions are good enough for the support of each and that the first wife does not lose her status.

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[1] This has been pointed out by Captain G. H. Pitt-Rivers in *The Clash of Culture* (1927).

V. THE FAMILY RELATIONSHIP.

We may now consider the relationship of parent and child. During infancy the only person besides itself of whom the child of either sex is aware is the mother. From her he receives all satisfactions and disappointments, and maternal love will cause the former to prevail. It is inconceivable that the newborn infant is aware of its own sex, *i.e.* that it should know whether it is of the same sex as or of opposite sex to its mother, but the emotional response to sensual satisfaction or disappointment is displayed at once. This, however, is undifferentiated, and thus for some time emotions develop in the infant of both sexes in relationship to the mother only. In later infancy or early childhood the father enters the child's life; his influence over the child will vary considerably, both according to the type of society and to the personal character of the father, for parental sentiment in the male has neither the force nor the stable character of maternal love. In late infancy the child recognizes that it gets a different response from persons who are like its mother, *i.e.* females, and persons who are unlike, *i.e.* males. At what stage it recognizes to which sex it belongs itself it is impossible to say. It is, however, observable that very young children, perhaps still in infancy, do respond differently to the sexes, though there may be much variation in the age and the degree of the response. At this stage it may be said that the child responds to sex according to its physical and mental capacity. Both for the boy and the girl there may be some rivalry with the male parent, for the father interferes with the infant's complete possession of the mother. For the male infant he is the prototype of the adult male, *i.e.* that person of the same sex, apparently endowed with unlimited power (for the child does not differentiate between the possible and the impossible in the vast category lying beyond his own power). For the girl child there is a difference: she has already become accustomed to the adult of her own sex, who is certainly omnipotent but on whom she, as well as her brother, can exercise all her infantile power. When the little girl first becomes aware of her father as someone different from her mother a new situation arises. Rivalry between the daughter and her father for the sole attention of the mother may exist, but it is probably more easily overcome by her than by her brother, owing to a new interest that arises, *i.e.* that of exercising the power, which she has already acquired over her mother, on a creature quite different from her mother and hence from herself, although this difference may perhaps only be realized later. This interest, charged at present with only infantile emotions, is actually the beginning of sexual attraction.

Here, in order to make clear my attitude, I may be permitted to insert a passage from a previous publication:

> In considering the attachment of the primitive infant to its mother we must not picture it as the baby in our own civilization, lying separated from its mother in cot or perambulator, becoming accustomed to the touch of many different hands and the sound of diverse voices, soon to find pleasure in hugging Teddy Bears and being distracted by bright-coloured toys. Comparison to the marsupial cub would really be closer, for the infant lies close to the mother's body, suspended in skins by day and huddled in her arms at night, never far from the nipple. Even at the crawling and toddling stage the primitive child can seldom be left alone, and must spend a large part of its time balanced on its mother's hip. The effect of this on the mental make-up cannot be ignored; there can be few memories of infancy and early childhood that are not associated with the mother, and it must be repeated that this condition is true for both sexes[1].

As the child passes from infancy to childhood social conditions will modify its behaviour. In early childhood the benefits received from its parents as well as their powers foster an attitude of love and respect. In later childhood the desire for independence will run counter to this, but need not greatly disturb it. At this period there is a tendency for girls and boys to divide into gangs of one sex, so that there often seems a natural separation between brothers and sisters; it is, however, doubtful whether this is enforced in savage society, while there is usually no check on sexual curiosity, childish investigation and satisfaction. Later, when adolescence is reached, there is often some form of initiation or puberty rite, during which sexual as well as other education is given, and as a rule after this period courtship and love-making begin. The youth has by now been taught with whom intercourse is permissible and with whom it is not. It is of advantage that the lines should be drawn strictly—at least in theory—and the extension of the terms that have already been associated with a definite form of behaviour within the family, upheld by sanctions that are readily accepted, are useful in regulating conduct in the larger community in which the adolescent finds himself.

The attitude of respect gained in childhood is of the greatest advantage to the group; sexual relationship between parent and child arising later would be dangerous to the family group (quite apart from the directly sexual jealousies that such a situation might cause), for the child would then be raised to the social level of the parent, who could no longer exercise the authority befitting the senior generation. From this point of view intercourse between father and daughter would upset the parent-child relationship and disturb the balance of the family group, just as much as intercourse between son and mother or between son and his father's other wives. Stress must be laid on this, for most theories of incest deal only with the jealousy that would arise between father and son if the son were allowed sexual freedom within the family group. While it is easy to see that even in the absence of a law with supernormal sanction the father would prevail and prevent such intercourse until the son became as strong as he, and that the son in turn would show no pity to the father, it has never been shown how a woman could prevent intercourse between her husband and her own daughter, nor indeed why such intercourse should appear in so heinous a light, since no objection is made in savage society by a middle-aged woman to her husband taking another wife as young as her own daughter. But if the prohibition of incest is looked upon as a social law regulating the behaviour of an entire social group (the family), rather than as a rule that gives old men sanction for checking the desires of their sons, the avoidance of both types of parent-child incest can be understood as safeguards to parental authority.

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[1] B. Z. Seligman, "Incest and Descent: their Influence on Social Organization," *J. Roy. Anthrop. Inst.* 1929, LIX, 242.

The parent-child type of incest, socially regarded, is *the law that prevents one person marrying two spouses who stand to one another in the relationship of parent and child.* The value of the parent-child incest law to the family group is very great, for the authority gained naturally by the parents over the children during the period of their complete dependence is thereby sustained indefinitely. No doubt adolescents desire to exert their independence, but should they at the same time be able easily to satisfy their sexual desires within the family, and thus dispossess one of their parents, the blow to parental authority would be such that the family group could hardly survive. If, on the other hand, a social law can be accepted which at the same time protects the mother from the son and the daughter from the father, the authority of both parents can be maintained over the children until they seek mates outside the family and themselves gain the social status of seniors without encroaching on that of their parents. The father keeps his authority over the family by accepting a rule that deprives him of intercourse with his daughter. In doing so the father gives up a conscious advantage, for there can be no doubt that young girls are attractive to mature men. Whether the women in accepting the parent incest barrier give up a conscious advantage, or only a fantasy arising from unconscious infantile emotion, may be questioned, but they certainly also stand to gain by retaining their maternal sway over the household.

From the adult point of view the question is largely a social one, from that of the rising generation it is psychological. Whether the Freudian hypothesis be accepted in its entirety or not, few people can doubt that there is such a thing as infantile sensuality[1], and that fresh associations made during life will follow the lines of the earliest associations. Thus the adolescent sexual attachments will follow patterns formed by prescribed infantile attachments. There is, at least in the unconscious, such a thing as a parent-incest situation. In the healthy adolescent the parent image is, however, replaced by a substitute (and there is no reason to regard the savage as a neurotic suffering from a parent fixation), a substitute who is far more likely to be young than of the age of the parent, so that in accepting the taboo on parent-child incest and so prolonging their own subjection to their parents it is doubtful whether savage children renounce any conscious desire at all, but merely retain in the unconscious an attitude towards the parents which at first was only charged with infantile sensuality and now has been reinforced by adolescence.

When the father accepted a barrier between himself and his daughter it is unlikely that he would permit his sons to take the very mates whom social pressure had caused him to renounce. The triumph of the sons over the father in mating with the sister would be unbearable, and considering the probable physical immaturity of the latter it would be unlikely that they could succeed without their father's consent. If the fathers renounce all sexual approach to their daughters the sons must do so too[2]. Thus the brother-sister taboo imposed upon the younger generation by the father, not only prevents rivalry between brother and brother and between sister and sister, but it removes a second sphere of rivalry between father and son. The two incest taboos are therefore complementary to one another, and together eliminate certain possible sources of disharmony within the family group.

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[1] I have used the expression 'infantile sensuality' rather than 'infantile sexuality, found in psycho-analytic literature, because to many people it is incongruous to speak of sexuality so long before genital maturity.

[2] Compensation in work or kind paid to the bride's father is a factor of great importance in savage marriage. Here we see an element that adds to the stability of marriage, that of property. It may well be that the development of this factor has buttressed both the father-daughter incest taboo and that between brother and sister, though it is by no means its only function. In actual fact the bride-wealth is almost invariably received by the bride's father, but very frequently is ear-marked to provide a bride for the son. Thus the brother has a great interest in his sister and is often looked upon as her guardian; the bride-wealth acts as a guarantee for the stability of marriage and marks the legality of the children. The situation between brother and sister is thus a delicate one; ordinarily she provides a wife for him, often he may feel real affection for her, so there is need for a definite behaviour pattern, which in many peoples is shown by ceremonial avoidance.

The incest barriers can be seen to have biological value, not by preventing inbreeding, but by forming strong groups capable of rearing strong offspring. While demonstrating the function of the incest laws, I do not put forward any theory of the origin of these laws beyond their biological survival value.

VI. DESCENT.

It is necessary to digress somewhat on the subject of descent, for though it is of great sociological importance its significance has been frequently misunderstood. Descent does not attach the infant to the group of his parents, birth and rearing do that; descent attaches a child to a definite social group. It is seldom that parental bonds are interfered with during the early years, so that the mode of reckoning descent probably has little influence on the young child in his earliest environment; but descent, authority, succession to office, occupation and the inheritance of property, are all very important matters which affect the individual in the later periods of childhood. In the earliest and simplest forms of society, however, these factors could not have been important. Children being brought up by the parents, the parents naturally exercised authority, taught the children all they knew, and left them what they possessed. It would probably be immaterial whether such valuables were taken by the sons (patrilineal inheritance) or by the daughters. The skill to use an object would be more important than mere possession, and this might be taught to sons and daughters or to the mates of daughters. Whether in such a state people were aware or not of the physiological meaning of fatherhood is unimportant[1]. What is now known as matrilineal descent and inheritance could hardly occur in the simplest society, for that implies a close bond throughout life between the respective families of brother and sister, consciously fostered by custom, so that the child of the sister becomes a man's heir. Such a condition implies a considerable degree of social organization, and is usually associated with clan organization. In matrilineal society to-day the young child is brought up with its real parents, and Malinowski has shown how the ignorance of physical paternity in the Trobriands does not interfere with the affectionate interest a man takes in his children, so that even in a highly organized matrilineal culture this feature does not affect the young child.

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[1] Malinowski, *Sex and Repression in Savage Society* (1927); Ernest Jones, "Mother-right and the sexual ignorance of savages," *Internat. J. Psycho-Analysis*, 1925, VI, part 2, 109.

Thus, in primitive society when social organization has not passed beyond the stage of the consolidation of the family, and to the child under four years of age in *any* state of society, both the form of descent and the real understanding of paternity are immaterial.

It is not, however, only in matrilineal society that paternity is disregarded. Even when paternity is well known, sociological rather than physiological factors often determine the descent of a child. Among many of the African patrilineal peoples it is neither marriage, maternity, nor paternity that clinches the matter, but the payment of the bride-wealth. Thus, among the Shilluk, in questions of divorce the descent of the child depends entirely on the payment of the bride-wealth. Among the Bari, should a man marry his mother's brother's widow without exchange of bride-wealth—a thing sometimes done when there are many widows and the heirs have already been satisfied—the man is not father to his own child by this woman but cross-cousin. In matters of authority and property the dead man's heir counts as the child's father. These are special examples of an attitude towards descent widely current in Africa. Adoption, so common in Melanesia, is further evidence of the sociological attitude towards parenthood.

In larger groups than the individual family a new situation is seen. It may be supposed that as soon as the struggle within the family had been solved by the imposition of the incest barrier, and economic conditions were favourable, larger groups would tend to form. These may have been on any basis, both sons and/or daughters remaining close to the parental sphere, or the brothers working together and settling together.

Such indefinite groups would, however, tend to split in any direction, and would never form such a stable foundation for the building up of custom and tradition as a group founded on some kind of descent. If it did not split (though economic need really precludes such a possibility) a community would find itself eventually related to the whole known world. *Thus every form of descent is limited, either naturally or artificially.* An artificial limitation has great advantages, as a natural limitation is apt to be brought about by strife or enforced migration. Indeed throughout the world we find an artificial limitation to descent. For a long time anthropologists only recognized matrilineal and patrilineal descent (and probably only noticed the former because it seemed 'so unnatural,' not realizing that the patrilineal descent to which we are accustomed is equally artificial); argument about their respective priority obscured further understanding. Malinowski was perhaps the first to clear the issue by insisting on the social importance of that group from which descent is not traced.

From the study of Deacon's work on Ambrym[1] I gained a further understanding of descent and hence the key to all marriage regulations. It has already been pointed out that all methods of reckoning descent are conventional except the equal bilateral system, in which relationship to both parents is regarded as of equal value. This system has, however, great disadvantages, for one has only to go back four generations to find oneself related to sixteen different family groups, with kinship ties to them all, while another generation doubles the number. The burden of such a method when associated with the classificatory system would be intolerable unless some limit were put to the number of generations forming a kindred group, or the extension of the idea of incest given up and endogamy allowed. As a matter of fact, both these solutions have been reached among different peoples. Other obvious conventions are the limitations of descent to the line of either one parent or the other (matrilineal or patrilineal descent). In the six-class system of Ambrym described by Deacon I was able to work out two other types of convention, and in that of Pentecost a quite unexpected development led me to recognize another form. I have been able to define (1) bilateral descent with matrilineal dominance[2], (2) bilateral descent with patrilineal dominance, (3) asymmetrical descent with matrilineal dominance (*i.e.* a system in which the two sexes do not reckon descent in the same way), (4) asymmetrical descent with patrilineal dominance, making in all seven methods of reckoning descent[3].

By means of a recognized form of descent a social group that is founded on kinship and is larger than the family gains the social solidarity that is necessary for it in the struggle for existence.

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[1] "The regulation of marriage in Ambrym," *J. Roy. Anthrop. Inst.* 1927, LVII.

[2] Types 1 and 2 are characterized by segmentation into groups in which each generation is separated from its suceeding and preceding generation and associated with the alternate generation.

[3] See my papers, "Bilateral descent and the formation of marriage classes," *J. Roy. Anthrop. Inst.* 1927, LVII; "Asymmetry in descent, with special reference to Pentecost," *ibid.* 1928, LVIII; "Incest and descent, their influence on social organization," *ibid.* 1929, LIX. The theoretical possibility of an eighth form of descent, by which males are descended from the father and females from the mother, should further be granted.

VII. MARRIAGE PROHIBITIONS.

Exogamy is an extension of the brother-sister incest prohibition on a unilateral basis. Anyone whose status resembles that of the sister is theoretically non-marriageable. The behaviour towards the own sister is observed in a more attenuated form towards all the large group of women called 'sister,' extended and limited by the conventional law of descent accepted in the society.

With matrilineal descent marriage between the children of two sisters is forbidden, but not that between the children of two brothers or of brother and sister, while with patrilineal descent marriage is forbidden between the children of two brothers.

Exogamy prohibits one type of parent union but not both; exogamy, however, never stands alone, and union with both parents is always forbidden. Frequently clansfolk of both parents are forbidden in marriage, and sometimes the clansfolk of the four related clans. Sometimes the clan of the parent through whom descent is traced is forbidden, and all near relatives of the other parent, the interpretation of 'near' varying in different societies. Or society may be divided into two moieties (dual organization), and with either matrilineal or patrilineal descent all ortho-cousins are forbidden, while cross-cousins are considered suitable mates. In some cases all relatives, or indeed the whole community, fall into more complicated groups, and the laws that regulate them appear arbitrary. Thus, out of the twenty types of second cousin which every individual can claim, marriage with only certain types is allowed and this is so in the famous Australian four- and eight-class systems. Marriage is here regulated by the less obvious methods of descent mentioned above. Once the systems of descent have been recognized it will be seen that the vast majority of marriage prohibitions fall into line as extensions of the brother-sister taboo, dependent on the particular convention of descent accepted in each society.

There is another class of marriage regulation, which does not fall under the brother-sister taboo. In order to understand these we must return to our definition of the parent-child type of incest. I have defined this as *the law that prevents one person marrying two spouses who stand to one another in the relationship of parent and child*, for this includes the fact that neither a man nor a woman may marry his or her own child and the extension of the taboo to the polygamous family, as well as those other marriage prohibitions—to be considered presently—which with those founded on the brother-sister taboo cover the whole field of marriage regulations.

I have suggested that the parent-child incest barrier in all grades of civilization as we see it to-day, and presumably in primitive society, is not a device that merely checks the lust and savagery of father and son, but one that, coupled with the brother-sister barrier, makes possible the formation of workable groups—first the family and then the clan—and lays the foundation for groups of all kinds in more complex societies. A careful examination of jealousy may give weight to my contention; despite the common belief that this is a natural adjunct to love, it is not at all safe to predict its absence or presence either among savages or among so-called civilized peoples. Jealousy is undoubtedly strengthened or weakened by conventional public opinion. Customs of exchange of wives, rights of access of diverse kinds so frequently found among savages, have attracted so much attention that it was considered by some writers that savage love was mere lust, in which personal choice, fidelity and romance were unknown. More careful observation shows that this is entirely untrue. I have suggested (p. 257) a possible biological reason for the modification of female jealousy. There are at the present day many millions of women who *in their own social milieu* lead a normal life with the certain knowledge that they are not the sole possessors of their husband's love; many a middle-aged woman takes an affectionate interest in a co-wife young enough to be her daughter. Why then the impossibility of marrying both mother and daughter, as seen in the father-daughter incest law and in its extensions to marriage prohibitions? Jealousy cannot be the only factor, and I must again emphasize the danger of upsetting the social distinctions between the generations, as well as call attention to that subtle but important relationship of 'in-law.'

If this is granted it is easy to understand the carrying over of the father-daughter incest prohibition to the widespread rule that a man must not marry a woman related to the first wife as daughter (either really or in the classificatory sense), as well as the elaborate precautions that are taken to prevent relations between a man and his wife's mother. This is not the only factor in 'mother-in-law avoidance'; suffice it to say that if the sexual aspect of the mother-in-law taboo[1] is looked upon as a safeguard against breaking down the distinctions of generations, it is not one of the ridiculous or barbarous customs of the savage, but falls into line with other regulations which help to secure the solidarity of the group.

Having examined briefly those marriage prohibitions that are extensions of the brother-sister incest and the father-daughter incest barriers, we must return to the mother-son incest barrier, or the law that *one woman may not marry two men related to each other as father and son.*

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[1] For other aspects of the taboo, see pp. 269, 270.

The usual extension of the mother-son incest barrier—besides its extension to all who are called 'mother' in the classificatory sense—is the prohibition against a man marrying his father's widows. It must be noted that there is no convention in savage life to safeguard the mother-son relationship. It is true that during initiation ceremonies the separation between mother and son is rigid, the mother among some peoples keeping up the fiction that the son is dead; but this, where it occurs, usually lasts for a short period.

Yet, according to the findings of psycho-analysis, it is the mother-son relationship that is fraught with so many difficulties and the solution of which does so much to form a man's true character. How is it that this situation appears to affect the savage so little? As in normal savage life the mother is not avoided, so women who are addressed as 'mother,' though unsuitable as mates, are not protected by any precautions to avoid them. It is not a satisfactory answer to the question to say that the conflict must be so deeply repressed that it is entirely unconscious, because other conflicts, which among ourselves may be relegated to the unconscious, show clearly in savage behaviour even if the motives are not consciously recognized. That indeed is the main difference between the savage and the higher cultures: in the former the conscious and unconscious are not so strictly separated; motives that are not consciously recognized get free expression in symbolic and ritual behaviour in ordinary life, so that it is unlikely that a desire would ever become so deeply repressed in savage cultures as among ourselves. Does the savage mode of life have a different effect on the mother-son attachment to that found among Europeans? Certain factors in ordinary life may make it easier for the savage to solve his parent complex and harder for him to solve that of brother and sister as compared with the European, so that for him the real conflict lies in the brother-sister relationship, whether the maternal or paternal pattern of descent be accepted.

Owing to the common taboo on coitus during infancy, the possession of the mother by the infant is more complete than among the civilized; he probably feels no shocks of jealousy, due either to the presence of the father or to the birth of another child, until he is well over two years old and often for much longer[1]. Ordinarily the father, as well as the mother, is indulgent; weaning is usually so gradual a process as to be hardly perceptible, and neither parent appears to interfere with the child's earliest sensual or curiosity impulses. When of an age for companionship with children of the opposite sex no guilt is usually attached to his play; he is able to satisfy his curiosity and desires with a sister or sister substitute, so at this stage he is not thrown back in fantasy to his infantile attachment to his mother. His attachment passes without any checks from the mother to the sister or a substitute, and continues until adolescence.

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[1] The question of the taboo on coitus requires further investigation. Among some people it appears to be kept stringently, while among others only penetration is avoided during the long lactation period.

An example from the Azande, given me by Dr Evans-Pritchard, illustrates behaviour in a comparatively advanced patrilineal society. The Azande have a saying, "Desire of women begins with the sister." Children of about six or seven begin to play at houses, where they go in couples and behave as grown-ups, especially imitating the sexual activities. If the father discovers that his son is indulging in this sort of play—and apparently his first efforts are often with a sister—he beats him, but it is especially the incestuous aspect of the act that is punished, and the child begins to learn, first that incest is reprehensible, and then, from the talk that he hears as well as from advice given him by his mother, that adultery is punished in the most terrible way by castration. This teaching points out what is and what is not permissible, but does not repress the idea of sexual development.

The absence of a latency period, so well described by Malinowski for the Trobriand Islanders and borne out by Evans-Pritchard[1] in a very differently organized society, is perhaps characteristic of most savages and has psychological importance. About the time of puberty the savage child receives a violent check; if he has not already been taught in childhood he must now learn to make love only to girls who, though of his sister's age, are not 'sisters' even in a remote degree. He may, or may not, have been prepared for this by sister avoidance—frequently there has been no such custom. Eventually, after a considerable amount of love and courtship, a mate is chosen and a new phase of life is entered. With marriage he has to start relationship with a new family group, indeed in a new clan; he must step warily; not only is a good relationship to his wife necessary, but to her whole family group, for should he offend any of them the reaction would be felt not merely by himself but by his group. Thus he stands as an intermediary between two groups, each of which for certain purposes is capable of acting in unison. It is towards the elder generation that he must be most on his guard; he must show them respect, and any hint of familiarity might be taken amiss; therefore a conventional ceremonial attitude is of great value. Any breakdown of the barrier between the generations is a danger to savage society. There seems to be a social necessity to retain the authority of the elders, who are the storehouse of traditional knowledge, even when their physical powers have begun to decrease.

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[1] Unpublished material.

Another factor in the behaviour towards the wife's parents is potent: every savage (man or woman) wants offspring, whether these are eventually to belong to his own group or to that of the spouse (according to the convention of descent accepted among his people), or even if, as with some forms of descent, they should belong to neither. He himself keeps in touch with his own ancestral spirits, who are always interested in the continuation of the line (whether reincarnation is believed in or not), and he will naturally look to them for favour with regard to his offspring. But the children are not his alone, they also belong to the line of his spouse, and the curse of an ancestor in the wife's line may cause the children to die or the wife to be barren. The favour of ancestral spirits on both sides of the family is needed, hence an added fear of the parents-in-law. Another factor is the economic one: there is usually an exchange of gifts at marriage, and this is a matter that must, if possible, be arranged without ill feeling. It must be remembered that it is not only the mother-in-law who is treated with ceremonial respect and avoidance; the attitude is extended to many others, and varies from tribe to tribe, but frequently occurs between persons of the same sex related by marriage. Therefore, though the sex taboo enters into the matter of mother-in-law avoidance[1] it does not cover the whole ground. Rigid taboos and prescribed ceremonial behaviour safeguard the situation.

Conditions of a different kind further link the mother-in-law taboo with the prohibition from marrying mother and daughter (*i.e.* the father-daughter type of incest). Sometimes a man or boy may be betrothed to a child in arms, or be given the promiseof a child yet unborn; in such cases he must wait many years for his bride, and during all that time he must treat the prospective mother-in-law with respect. In such cases the mother-in-law taboo is practical, for the temptation may be real. Infant betrothal is, however, only one element in the mother-in-law relationship and must not be regarded as accounting for its inception.

In distinction to the suggestion that the mother-in-law attitude is associated with the father-daughter incest barrier, is the theory put forward by Freud that it is an extension of the mother-son incest barrier.

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[1] M. Mauss, "Parentés à Plaisanteries," *École pratique des Hautes Études, Section des Sciences Religieuses, Annuaire*, 1927, 1928.

He sees in the attitude to the mother-in-law a revival of the mother-son incest attitude, and looks upon avoidance as a repetition of the behaviour adopted to avoid incest with the mother. The objections to this theory are that, whereas the mother-in-law behaviour pattern is widespread, avoidance of the mother is rare, and in savage society the son does not enter his wife's family to repeat in it a situation which he previously held in his own, but when he marries he begins a new relationship to a new social group, and undertakes new responsibilities.

We may now review the main marriage prohibitions found throughout the world. They include the parent-child and brother-sister incest taboo, all other prohibitions are derived from these and are extended by means of (1) clan exogamy with matrilineal or patrilineal descent; (2) dual organization with matrilineal or patrilineal descent; (3) kindred grouping with bilateral descent; (4) four, eight, and also six-class groups with bilateral descent and either matrilineal or patrilineal dominance; and (5) three and six-class groups, where brother and sister may not marry sister and brother (the rules of descent being different for men and for women).

Of all these prohibitions only those founded on kindred grouping prevent the marriage of persons related by blood. All the other prohibitions work in entirely arbitrary ways, dividing up the cousins, according to the recognized method of tracing descent, into marriageable or non-marriageable. It has been shown that these prohibitions are extensions of the rule that true brother and sister must not mate, and the particular pattern is dependent on the type of descent adopted in each form of society. This formidable list of regulations testifies to the strength of the brother-sister temptation and the importance attached to the prohibition. Regulations dependent on the parent-child barrier also exist and have been mentioned, but they are less numerous; moreover, as will be seen presently, regulations at variance with these rules are widespread and appear to function smoothly.

VIII. EXCEPTIONS TO THE RULE, AND EXCEPTIONAL REGULATIONS.

A further method of examining the strength of the incest barriers is to consider the exceptions and see which type they follow predominantly. These fall into four categories:

(1) Those exceptions to the rule when the individual knows that he is transgressing but cannot resist the temptation.

(2) Among certain peoples there are privileged classes who are allowed to enter into unions which are taboo to the rest of the community.

(3) Among certain peoples unions are legally formed which are not allowed generally and are looked upon askance by neighbouring tribes.

(4) Exceptions for magical purposes.

(1) It is difficult to give any exact estimate of breaches of the law, but where the observer has had sufficient knowledge of the people to investigate the matter, a large number of brother-sister unions—true sister, half-sister, and the classificatory sister—have come to light, while records of parent-child unions have been comparatively rare[1].

(2) It has long been known that the Azande royal clan (the Avungara) has disregarded all restrictions on marriage, and Dr Evans-Pritchard has recently made an intensive investigation of these people. The Avungara do not consider themselves bound by any restrictions at all, unions between children of the same father and of different mothers are the rule, and unions between father and daughter are common; Dr Evans-Pritchard only heard of one union between children who were considered full brother and sister; moreover this chief suffered from leprosy—the punishment that is believed to fall on commoners who infringe the rules of incest or exogamy. Even this couple were not true brother and sister, but children of one father, their respective mothers being sisters.

I cannot here discuss the social and economic significance of the Avungara marriages, but it must be understood that the chiefs take a large number of commoner wives as well as their relative wives, and that the former bear most of the children; nor are the children of the brother-sister unions specially regarded as heirs. The incestuous marriages practised by the Avungara give the men entire freedom of choice, and they generally say that they marry their sisters because they consider them good-looking; their children take no precedence over the children of commoner wives. The women living in the wealthy households of their fathers and brothers are freed from all work and responsibility, so far as possible they shirk the duty of bearing children, and are notorious for their Lesbian practices. Both parties are freed from the laborious duties to 'in-laws' usually involved in ordinary marriage.

Thus the marriage of the Avungara woman has not the stability of that of the commoners, indeed her position is not very different from that of a Shilluk princess, who is not allowed to marry but may take lovers as she pleases; incest is not forbidden, but as, among the Shilluk, children are never brought up in the royal household and two royal children are not brought up in the same village, no special temptation to incest exists.

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[1] See Malinowski (*Sex and Repression in Savage Society*), who notes a large number of breaches of the brother-sister incest law, one case of incest between father and daughter, and none between mother and son. See also Raymond Firth ("Marriage and the classificatory system of relationship," *J. Roy. Anthrop. Inst.* LX,) and unpublished notes by E. E. Evans-Pritchard.

The marriages of the royal family in Ancient Egypt are no guide, as these were doubtless actuated more by the desirability of finding a suitable mate for the heir apparent than from free choice. The same would probably apply to the royal family of Burmah, who until recent times practised incestuous unions—chiefly between half-brothers and half-sisters.

(3) The remarriage of widows falls into the third class of incestuous union. In savage society it is important that everyone should have a definite social status, and the status of widows requires economic and social consolidation. As a rule, especially where there has been an exchange of bride-wealth, unless she is considered wife to the deceased even after his death, the widow belongs to the group of her husband and her maintenance becomes the duty of the heir, this being usually solved by marriage with her. If the woman is old the marriage may be entirely formal, but the woman has a place in the household of the heir. If she is young—and with polygamy it often happens so—the heir actually marries her unless she raises objections and prefers some other man, when the arrangements for the marriage are made by the heir, and all customary exchange of bride-wealth is with him.

When inheritance passes in the maternal line, and descent is also matrilineal, such marriages do not transgress the rules of incest or exogamy. The heir, the sister's son of the deceased, is necessarily of a different clan, and as a rule he has not addressed the wife of his maternal uncle by a term that is associated with the mother.

Where descent and inheritance is patrilineal the arrangement is similar, though it looks very different, for the widows are inherited by the sons. No man will ever marry his own mother, and if she does not wish to remarry arrangements will probably be made for her to live in his household, but among many peoples, especially in Africa, he does marry the father's other widows, unless they wish to make other arrangements, in which case he must receive the bride-wealth. While the father lived these women were addressed as 'mother,' yet the change of status seems to cause no shock. Among the Shilluk, where this is the usual practice, a further custom occurs which seems to break all rules. The Shilluk allow wide rights of access; a man may go to the wife of any man whom he calls 'father's son,' as well as to the wife of any man whom he calls 'father,' except his own mother. When asked whether the father did not object, my informant replied that he might, but that he could do nothing, and it was better for him that this should happen than that his son should commit adultery, for then the weight of the very heavy customary fine would fall mainly on the father; further, a father who took young wives to himself before helping his adult son to get a wife would be looked upon askance. In several other African patrilineal tribes the son is allowed to cohabit with the younger wives of his father when the latter is old, though perhaps among most peoples the privilege is accepted as a favour, not taken as a right as among the Shilluk.

With matrilineal descent and inheritance, and matrilocal marriage, another type of infringement of the general rules may occur. Among some of the tribes of Assam, where the woman herself is the heir and her husband her executor, if his wife dies the widower must marry the next woman of kin so as to administer the property, even if the next of kin is his wife's mother or her daughter. Among some American tribes, for similar reasons, a man may marry a woman and her daughter.

The remarriage of widows is regulated by the type of descent recognized, and by the attitude towards property. It is remarkable that among some patrilineal peoples the association of the wives of the father with the mother, and the fact that these women have previously been addressed as 'mother,' does not prevent them from being legally taken as wives. It may be suggested that the social necessity of remarriage for the widows overweighs any other consideration, but this could not be so among people who were obsessed by a fear of incest with the mother.

(4) The examples that I have collected in the last category, *i.e.* exceptions for magical purposes, all come from Africa, and they include both the brother-sister type and the father-daughter type, but the brother-sister type seems to predominate.

Considering these exceptional cases—both those that are breaches of law and those that conform to special law—we have more evidence for the occurrence of brother-sister incestuous unions than for parent-child unions. Father-daughter incest occurs both with matrilineal and patrilineal descent (in the first case where it is not a breach of exogamy, in the second where it is). Mother-son incest is only condoned for the purpose of succession, as in Ancient Egypt, and has very rarely been observed. Among many peoples the remarriage of widows who stand in the mother-son relationship occurs, and the situation does not appear shocking, so that it may be assumed that it does not stir up latent feelings of guilt towards the mother.

Our examination of savage laws and customs has thus led us to the conclusion that the problem of brother-sister incest has neither been solved nor relegated entirely to the unconscious. In all except endogamous peoples it is an important factor in conscious behaviour, and accounts for the most complicated marriage regulations. The same does not appear to be true of the parent-child incest problem.

IX. THE VALUE OF MARRIAGE PROHIBITIONS.

We may now ask, what is the value of the marriage prohibitions? There can be no doubt of the great cultural and survival value of the incest prohibitions, the three laws being accepted as complementary to each other. By this means the adolescents remain within the group to strengthen it and improve its capacity for action, especially to increase the food supply. There is perhaps a further biological factor, apart from the question of inbreeding, that may have survival value. A dysgenic factor in parent-child incest is that, one parent being considerably older than the other, the offspring may be left without adequate provision when the older one dies, while brother-sister incest would allow the mating of two immature partners, and the mere fact of the prohibition must lengthen the courting period and allow more chance of the mating couple becoming mature.

The extension of the incest laws beyond the family to marriage prohibitions presents a different problem. It must be clearly stated that exogamy has no biological advantage (if in-breeding is regarded as an evil). Exogamy encourages rather than discourages the marriage of near kin, for exogamy is always based on a conventional view of descent which disregards half the relatives. No one could seriously contend that it is more eugenic for the children of brother and sister to mate than for the children of two sisters or of two brothers, yet the former is the favoured marriage among a large number of exogamous peoples; the four-class system and the dual organization allow the same marriages of cross-cousins; the eight-class system does not allow the marriage of first cousins, but allows the marriage of certain second cousins and not of others; the tri-clan and six-class systems again differentiate the cousins in an entirely arbitrary way. On the social side the advantage is at first hard to realize, for it cannot be said that an exogamous clan is in actual fact a more harmonious social group than the endogamous Maori *hapu* or the endogamous Arab sub-tribe.

What then can be the reason for the survival of this hampering infringement on free choice? It can only be seen in an indirect way. The classificatory system of kinship carried the behaviour pattern developed within the family to the clan, and with regard to respect for elders and collaboration of kinsmen the advantage in social cohesion is very great indeed. The restrictions on marriage that are associated with this form of social structure cannot fail to be irksome, but with these very restrictions go advantages, for if certain groups of women are excluded as mates other groups are left over, and it is easy to claim that these women, being restricted from their own clansmen, are rightful mates. So, paradoxical as it may appear, the actual function of the marriage prohibitions seems to become not so much a restriction on choice, as the institution of a legal right to marry within certain groups.

X. COMPARISON OF THE INCEST BARRIER IN SAVAGE AND CIVILIZED SOCIETY.

If my thesis concerning the part played by the incest barrier in savage society is correct, then I may presume to draw an inference from it, dealing with our own society.

Western European civilization lays stress on the ignorance (so-called innocence) of children, chastity of women, monogamy and yet the fecundity of married life, and has encouraged a latency period and fixation of the infantile emotions on the parents. This type of culture encourages so strong an ambivalent attitude (love and revolt) towards the parents that either element may tend to become fixed on them or on substitutes, and the brothers and sisters grow up in comradeship in which as a rule there is but the slightest sexual element. As the adolescent love ideal is mainly founded on the attachment to the parents, not on that towards brothers and sisters, the protective and tender emotions in love are fostered.

In savage society, on the other hand, the child suffers less from emotional shocks at the hands of its parents; ideas of guilt are less strong, there is as a rule no check on childish sexual curiosity, which young children can satisfy with sisters or other children of similar age. This easy transition of the emotions from the parents to contemporaries allows the child to pass from parent to sister love, and so what may be called the rough and tumble element in the emotion is encouraged rather than the protective element.

The breakdown of the authority of parents and the loss of prestige of elders, which is in process to-day in all ranks of civilized society, may tend to alter the attitude towards incest and hence towards love; and though the results may not be entirely foreseen, in the rising generation the male protective element and male jealousy both appear to be less prominent than in the recent past.

XI. CONCLUSIONS.

Civilization is seen in the capacity of man to live in organized groups. To this end natural impulses become modified for social gains.

The family, the first human group, became consolidated by the acceptance of incest barriers.

These barriers were not consciously formulated, but arose, not specifically to check the lust of the *pater familias*, but in the production of an harmonious family group, and actually changed it from a natural to a social group.

The sanction for the incest barrier is primarily the persistence through childhood of the infant's belief in the omnipotence of the parents, later developed into a religious attitude and consolidated by rites expressive of ancestor worship.

The persisting family is a social group with such survival value that other societies which may have ignored the incest barrier have died out. The family group is the pattern on which all other groups are formed.

With the formation of the clan and the classificatory system of relationship the rules of incest became extended.

Descent is reckoned not on two conventional principles, but by seven methods; when these are recognized all marriage prohibitions can be seen as extensions of the two primary incest prohibitions, that between parent and child and that between brother and sister, and investigation shows that the latter is the more important in savage society. The restrictions on marriage can thus be looked upon in each society as a reflection of its people's fear of incest.

The solution of the familial conflict is influenced by social conditions, and savage life leaves the brother-sister relationship so unsolved that it is a matter of conscious concern, while the parent-child type, especially that between mother and son, usually presents no social problem.

(*Manuscript received* 19 *September*, 1931.)

PUBLICATIONS RECENTLY RECEIVED.

The Psychology of Clothes. By J. C. Flugel. London: The Hogarth Press, and the Institute of Psycho-Analysis. 1930. Pp. xv + 242. 21*s.* net.

Although Dr Flugel approaches and treats his task in a scientific spirit, he has produced a lively and amusing book, which once begun it is difficult to lay aside, no matter how much one may question some of the fundamental assumptions or disagree with the method of their application.

Accepting as basic the thesis that clothes serve three purposes—decoration, modesty and protection—and that of these decoration is the primary, the book begins by examining the relations between the three members of this trinity. The essential opposition between decoration and modesty is regarded as the most fundamental fact in the psychology of clothing, since it implies that from the start our attitude to clothes is inevitably ambivalent, clothes serving on the one hand to display our attractions, on the other to hide our shames. In so far as clothes represent a device for obtaining some degree of harmony between conflicting interests, their use from a psychological aspect is not unlike the process whereby a neurotic symptom is developed: to use the author's psycho-analytical analogy, they "resemble a perpetual blush upon the surface of humanity." When, as is the case in all civilized societies, the exhibitionist tendency to display is connected with the clothed and not with the naked body, it is far more possible to attain a relatively stable compromise in attitude between the conflicting tendencies, and from this it follows that the changes made manifest in a series of fashions represent minor disturbances and readjustments in the established compromise between the tendencies.

The book discusses in detail the main psychological and social purposes of decoration, and outlines the chief forms through which these purposes find expression. Succeeding chapters are concerned with types of dress and with the questions of modesty and protection. It is argued that perhaps the most protective function of clothes is to act as a defence against the general unfriendliness of the world as a whole—a reassurance against the lack of love. For example, a person's sensitivity to cold may be regulated by his feeling of a lack or sufficiency of love in his life! Hence too the fact that certain forms of clothing, *e.g.* a high neck-band or tall coat collar, have traditionally been identified with 'unapproachableness' or 'standoffishness.'

A brief indication of the manner in which the mental attitude to clothes develops in the early years, is followed by a provisional classification into 'types' of the individual differences in adults towards clothes, differences due to sex being reserved for special treatment. The most significant of the sex differences would appear to be those which depend upon the relative importance of the two motives of modesty and display, this relationship being greatly affected by the fact that the displacement of exhibitionism from the body to the clothes has gone much farther with man than with woman. Among women there is a tendency for the sexual libido to be more diffuse, which makes a complete symbolic substitution more difficult. It is thus possible for the same article of dress, *e.g.* a hat worn in church, to fulfil two contradictory functions, in the case of the male to serve as a type of decoration "contravening the essential elements of religious humility," in the case of the female as the modest covering of a disrespectful naked head.

After an elucidation of the connection between these factors and the tenets of 'nude culture,' there follows a provocative discussion of the causes of the "Great Masculine Renunciation"—the abandonment by man at the end of the eighteenth century of his claim to be considered one of nature's beauties. Since then, it is maintained, along with the growth of democracy and the increasing interest in the external world, man's clothing has become relatively uniform and standardized, symbolizing at once his devotion to the principles of duty, his renunciation and self-control, and, through the phallic attributes of the individual garments, the fundamental features of his sexual nature. There is a real connection on this view between the sudden great development of science at the beginning of the last century and the abandonment of ornamental clothing on the part of the male sex. A critic could speculate, without mere facetiousness, on the enormous consequences which might follow in the twentieth century were the female sex moved to a similar renunciation!

Another group of chapters is concerned with the evolution of garments, the forces of fashion and the specific forms through which they manifest themselves. In the author's view, the most important variations in fashion are due to changes in social opinion as to which feature of the feminine form is, at any given period, the special centre of erotic charm. This explains the successive rise and disappearance of the whole series of feminine fashions which range from the Renaissance dress with its emphasis on the abdomen, through the empire, crinoline and bustle, to the recent straight, short, sleeveless dress with its emphasis on the limbs. In view of the admittedly transitional character of contemporary feminine clothes, it would be interesting if one could evaluate with any real accuracy the weight which other and more rational factors will be likely to bear in directing the trend of this evolution.

Three chapters are devoted to a consideration of the ethics of dress, the principle adopted as the norm being the Freudian hedonistic one that clothes should be designed to secure the maximum of satisfaction in accordance with the 'reality principle.' The chapters include, perhaps inevitably, a discussion as to whether the two sexes should be dressed alike or differently, and the discussion is used to re-emphasize the implications inherent in the usual rigid differentiation. The establishment of an official Clothing Board or Ministry of Fashions is suggested, to ensure the efficient and scientific adaptation of costume to the spirit of the age, so that psychologist, artist and tailor may combine to make available for us the clothes we deserve.

In a final chapter concerned with the future of dress, the conclusion is reached that logically the maximum of satisfaction in accordance with the reality principle would be obtained by the abolition of all clothes, except perhaps for the sole purpose of protection. All lesser attempts at sartorial reform are in reality but compromises, and should be looked upon in that light.

Apart altogether from inevitable questions of practicability, the degree to which such an apparent *reductio ad absurdum* can be accepted, depends very largely upon an initial acceptance of the author's psycho-analytical outlook upon the whole subject. Doubts in this respect make it difficult to assess the psychological value of the book, or to decide what amount of new light it sheds upon the topics about which it so entertainingly and learnedly discourses. Despite admiration for its exhaustive treatment of a rather neglected problem, one may feel very strongly in reading it through, that to a psychologist not quite of Dr Flugel's persuasion, the relentlessly consistent lines of reasoning and the neat inevitability with which each fact seems to fit into its predetermined framework are sometimes just too good to be true! In an obscure (and possibly ignorant) way one may find oneself doubting whether, in sifting the evidence, enough attention was paid to the functioning of the casual and relatively undetermined factors which influence the form of our commonplace day to day life.

Dr Flugel opened his case by pointing out that it was the absence in the handbooks of psychology of any treatment of the social significance of clothes that required excuse and explanation: whatever their point of view, all psychologists are now indebted to him for an able and attractive elucidation of that important problem.

M. D. S.

Personality and Will. By F. AVELING. London: Nisbet and Cambridge University Press. 1931. Pp. x + 245. 5*s*. net.

The second volume of The Contemporary Library of Psychology has been written by its editor, and, as will be seen from its title, it concerns some of the most puzzling and intractable problems in the whole field of psychology. The library aims at presenting the problems of psychology "in a popular way, but at the same time without any loss of scientific accuracy." From this book it may safely be gathered that to the editor 'scientific accuracy' is a far more important and interesting ideal than popularity. Nobody is likely to find Prof. Aveling's arguments easy to read or to follow, and at the same time nobody can fail to admire the courage and sincerity with which he attacks the topics of his discussion.

The key to the book may be found in three brief quotations taken from the first chapter. Both personality and will are attested in immediate and 'intuitive' introspection. In such introspection we find not only mental objects and processes but "something holding them together; a self who contemplates or recalls the objects, who is pleased or unpleased, who is actuated by desire, moved by impulse and so forth. Such continuity and unity as may be asserted of the stream of conscious processes belongs to its several parts only because of the consciousness of the profound self as a unitary being, to which the processes successively belong, of which they are essentially the acts" (p. 20).

And again: "Continuity is secured by self-intuition, always identically and essentially the same. It is one and the same self to which the discontinuous stream belongs; and it is immediately known to us as being self-identical" (p. 21). And again: "The proof of continuity and unity does not require that full self-awareness should never be interrupted; it requires merely that whenever we are aware of self we should be aware of it as essentially the same in nature, and that we should refer to it all our experiences" (p. 22). These quotations come from a chapter entitled "General Setting of the Problem." The next three chapters are all historical and discuss problems of personality and will as they were dealt with in general theorizing from pre-Socratic thinkers to James and Wundt. It is a pity that so much space in a small book is given to this, for all the doctrines dealt with have been summarized again and again and Prof. Aveling's account, clear, concise and judicious as it is, is no better than many others. Next comes a treatment of experimental investigations, concerned mainly with the work of Ach, Michotte and the laboratory of which Prof. Aveling is the Director. This is intensely interesting and might with advantage have been further extended. There is a brief account also of the statistical approach attempted by Webb, Lankes and others. Again it becomes clear that the evidence accepted by the author for personality and will rests solely upon a peculiar intuitive introspection in which subject and object are claimed to be literally one. The book now proceeds to some study of comparative psychology, and of the development of sentiments, values and character. A final chapter sums up the position reached by a consideration of the values of personality in which reference is made briefly to many psycho-pathological studies. There are a great many bits of able exposition and there is much clever argument, but nothing is added to the central *assertion* of personality and will as real data of experience. The definition of personality that is finally reached, however, appears to contain more than this, for it runs: "Personality may be defined, not merely as the organisation of all so-called mental contents, traits, capacities and reaction tendencies, but as that final perfection which virtually includes in itself all the principles of human activity and constitutes a unitary, self-conscious, and (to some extent at least) self-determining individual." Even if the reader succeeds in getting clear to himself precisely all that this may mean, he will probably have some difficulty in admitting its justification. For the intuitive introspection upon which it is all based seems to be singularly devoid of any discoverable content. At the best it merely gives us evidence that there is something active in certain human reactions, in itself effortless but often preceded and followed by felt effort, which we may, if we choose, call will or self. But it is hard to resist the conclusion that all the descriptive characteristics of this activity, and all its consequences and functions, are attributed to it on the basis of speculative construction of a predominantly philosophical nature.

Nevertheless Prof. Aveling has produced a book that will provoke much serious thought. It is greatly to be hoped also that it may induce several competent experimenters to repeat the critical experiments of Ach, Michotte and others, and to get controlled introspections from subjects who have no knowledge whatever of the controversies that have raged around the notions of personality and volition, and even no pre-existing strong orientation towards any particular psychological or philosophical theories.

F. C. B.

The Physiology of Beauty. By ARTHUR SEWELL. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. ix + 194. 8*s.* 6*d.* net.

This book represents an attempt to get away from the influence of old philosophical and teleological speculations and to make use in the fields of ethics and aesthetics of concepts introduced by the work of Pavlov into biological science. The writer concludes, after examining its use by the philosophers, that the term 'reality' means nothing because it can mean anything according to the will of the thinker. "To say that a thing is real is to say nothing: to say that an event happened is to say all that we may say." Pavlov's dog therefore is your true philosopher, he concerns himself only with happenings and not with existences.

Pavlov's experimental results also supply the data which enable Sewell to assert that the apprehension of space and of the duration of time are conditioned stimuli; this leads to the dismissal of the word 'mind' from his vocabulary and, *ipso facto*, to the dematerialization of 'matter.' The scientist's world becomes then, the world of behaviours and differences, the things which are the resisting part of his universe. In this universe there is found, however, a division within the activity of the self which Sewell calls the 'public' or communicable and the 'private' or incommunicable. It is the 'public' world, the world of happenings or behaviour on the part of all things whether animate or inanimate, that forms the subject matter of science, and is the only true 'reality.' The mechanism of every kind of communicability is the conditioned response, language being considered in this respect as a remotely conditioned substitute for a conditioned stimulus. To the class 'private' or incommunicable, belong all those things which cannot be refined into terms of structure and behaviour, concepts like purpose or beauty or God, things which have "an utterly private quality of response" and are enjoyed and not discriminated, about which therefore, no 'public' generalizations can be made.

Having elucidated this principle of division into the 'public' and the 'private,' Sewell goes on to discuss, in its light, the underlying bases and assumptions of idealist and realist ethics, his discussion of Prof. Moore's definition of the 'good' being of particular interest. His conclusion as a 'publicist,' is that the 'public world' has nothing to do with goodness, it can only be silent about it, because neither goodness, nor badness, nor value, nor freedom are to be found in behaviour with which alone the 'public world' is concerned. Moral judgments certainly have their validity, but this is of a private and particular kind, incapable of being justified in words, the vehicles of publicity.

His problem, so far as aesthetics is concerned, is to discover whether or not the classification into good and bad art is a classification established 'publicly' within the works or 'privately' within our responses. His arguments are based upon an extension of the results obtained by Pavlov in the effect of a 'neutral' stimulus on an inhibited conditioned response. An aesthetic judgment is found to be compounded both of 'public' and 'private' elements, our responses to art being of the nature of conditioned responses to conditioned stimuli, so that the art of each epoch is distinguished by the incidence of particular compound conditioned stimuli or situations. Our appreciation of any given work of art partakes of the nature of Pavlov's dis-inhibition and biological utility mechanisms.

The author makes some trenchant criticisms of the writings of Roger Fry, Clive Bell and I. A. Richards, and his book should stimulate discussion over a wide field. Prof. Hogben points out in his introduction that the speculations it embodies are not intended as the last word which biology has to say about the analysis of conscious behaviour, but that they represent an approach to a "new humanism," the subject matter of which is amenable to experimental enquiry. The book can be heartily recommended to anyone who enjoys a lively and at times almost disrespectful reconsideration of old problems from a fresh point of view.

The Will to Live. An outline of evolutionary psychology. By J. H. BADLEY. London: George Allen and Unwin, Ltd. 1931. Pp. xi + 267. 10*s*. 6*d*. net.

"A living organism is distinguished from things which we regard as not living by the presence of an integrative principle by which it is compelled, however complex it may be, to act as a unit." This quoted sentence will serve to explain the point of view from which the subject has been treated. It will also account for what, despite the author's explanation, one feels was a rather unfortunate choice of title, which tends in some way to disguise the value of a piece of scholarly analysis that deserves to be widely read. For the psychological outline contained in the book is distinguished by a rare lucidity of exposition, by an impartial weighing and sifting of varying sources of evidence, and by a catholicism of interest not always found in either the professional or the practising psychologist.

The book owes its origin to a series of psychological discussions which the author conducted from year to year with some of the senior pupils of the well-known coeducational school of which he is headmaster. The treatment therefore is relatively simple in character though by no means merely elementary. There is a good chapter on the elements of mental activity and the processes of psychological growth. Others deal with the usual topics of instinct, habit, thought, emotion and will, and the outline is rounded off by a necessarily brief but well-balanced consideration of such problems as those involved in complexes (including sentiments), conflict with its consequent defence mechanisms, and the subconscious mind, the latter containing a section on dreams. A useful list is given of books suitable for further study, for the convenience of those readers who may wish to follow up any special part of the subject for themselves.

As a sound and clear introductory study of a wide and complicated subject the book should prove invaluable to study groups or tutorial classes, and indeed to the ordinary reader and psychologists in general.

The Psychology of Children's Drawings. By HELGA ENG. London: Kegan Paul, Trench, Trübner and Co., Ltd. International Library of Psychology, etc. 1931. Pp. viii + 219. 12*s*. 6*d*. net.

Dr Eng has written an extremely interesting and valuable study, probably the best that has as yet appeared in its own field. The first part, which is profusely illustrated, deals with the drawings made by one child from the first stroke to those produced at eight years of age. The second part, which draws upon this and a great amount of other published material discusses the main steps, or phases, of development in the free drawings of children. A final section deals with the relation between children's drawings and folk-art material. There is a good bibliography. The whole is a clear and admirably concise bit of work which should attract much attention.

The Psychology of Intelligence and Will. By H. G. WYATT. London: Kegan Paul, Trench, Trübner and Co., Ltd. International Library of Psychology, etc. 1930. Pp. viii + 269. 12*s*. 6*d*. net.

The book contains three parts. The first gives a critical account of certain current views about intelligence and methods of testing intelligence, and in particular of the work of Thurstone, the Gestalt school, Thorndike, Spearman and some of the Behaviourists. The author quite truly points out that current psychology has no widely accepted view concerning the nature of intelligence. He himself is sure that intelligence is essentially a capacity to apprehend relations which are pertinent to some purpose or other. As to this there would be little dispute nowadays; but most psychologists would probably consider that such a description needs rather more careful and critical examination that it here receives. The second part of the volume is a brief discussion of the relation of instinct and intelligence, and its main issue is the contention that intelligence may rule or control instinctive tendencies, although it is not bound to do so. The third part deals with intelligence and volition. An account is given of a good many different theories of volition, and, somewhat more briefly, of current and recent experimental studies. The author considers volition to be a genuine fact of highly developed behaviour, not in any way reducible to anything else. Hence "Intelligence without volition is important; but volition without intelligence is impossible."

The essay is full of interest and presents many points for debate. The general impression left is that, although the author combats many modern solutions of his problems, his own position is not quite as outstandingly original as he seems to think.

Integrative Psychology: A Study of Unit Response. By W. M. MARSTON, C. D. KING and ELIZABETH H. MARSTON. London: Kegan Paul, Trench, Trübner and Co., Ltd. International Library of Psychology, etc. 1931. Pp. xvi + 558. 21*s*. net.

Psychology is a study of how the organism responds to environmental stimuli and situations. There are four types of fundamental unit responses: Dominance, Compliance, Submission and Inducement. It is impossible to describe here the ways in which these basic notions are applied. They are worked out in considerable detail, and with unfailing enthusiasm, in regard to some physiological mechanisms, to 'drives,' and to conscious responses. To put all the complex human responses in terms of the four unit reactions mentioned above may seem to be a striking instance of over simplification. But the authors do undoubtedly attain a consistency of treatment in this way which is genuinely interesting; and in the course of their treatment they discuss innumerable details with much clarity. The advanced student, or the teacher, will find much valuable work in this volume, and will gain stimulation from its study.

Genius and Creative Intelligence. By N. D. M. HIRSCH. Harvard: Sci. Art Publishers. 1931. $4.50.

There are three parts in this book. First, some account is given of a number of sociological theories having a biological background, the nature of 'creative adaptation' is discussed and its necessity emphasized. 'Creative adaptation' is defined as social "adjustment that is consciously and deliberately carried out for ideal ends." Secondly, "the psychological constitution of man" comes under review. The author is largely concerned with the relations of instinct and intelligence and with the practical functions of intelligence. Thirdly, the volume considers "the nature of genius," its relation to "creative intelligence" and its part in "creative adaptation." Not only are many interesting facts set forth, but Dr Hirsch shows a wide knowledge of historical and contemporary theories. The exposition is often somewhat jerky and a considerable number of the statements made lack clarity. Nevertheless there is much to be learned from this book.

Encephalitis Lethargica: its sequelae and treatment. By CONSTANTIN VON ECONOMO. Translated and adapted by K. O. NEWMAN. Oxford University Press. 1931. Pp. xvi + 200. 18*s*. net.

Encephalitis lethargica is known universally to be a disease having important but insufficiently understood psychological consequences. It is therefore a little disappointing to find that Prof. von Economo has not taken the opportunity afforded by this new and admirable translation to attempt a more thorough account and study of the psychological side of his topic. There is indeed a short section on "psychological disorders," consisting of six pages, but this is not very clear and shows no acquaintance with modern psychological methods of investigation. It is true that the author asserts that "every psychologist who in future attempts to deal with psychological phenomena, such as will, temperament and fundamentals of character, such as self-consciousness, the 'ego,' etc., and is not well acquainted with the appropriate observation on encephalitic patients, and does not read the descriptions of the psychological cases in the many original papers recording the several mental symptoms will build on sand." This may be a just observation, but Prof. von Economo does not himself do much to help the psychologist, save very indirectly, in this book. But on all other matters relating to encephalitis lethargica the book is, as is well known, a most valuable and monumental piece of work. The works appearing in the selected bibliographies and references are exceedingly well chosen.

Phantastica: Narcotic and Stimulating Drugs. By LOUIS LEWIN. Translated by P. H. A. WIRTH. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. xi + 335. 15*s*. net.

The many drugs dealt with are classified under the following heads: Euphorica: mental sedatives; Phantastica: hallucinating substances; Inhebriantia; Hypnotica: soporifics; Excitantia. An enormous amount of information is given, and there are a good many references to original authorities. The book is full of interest for the general reader, though its very wide scope inevitably makes it often dogmatic about disputable points. Still, as a work to stimulate interest and as an introduction to specialized study, this book should prove valuable.

Ritual Psycho-analytic Studies. By THEODOR REIK. Translated by DOUGLAS BRYAN. London: The Hogarth Press. International Psycho-Analytical Library. 1931. Pp. 366. 21*s*.

The topics dealt with are Couvade and the psychogenesis of the fear of retaliation; the puberty rites of savages; Kol Nidre, a part of the Jewish rites of atonement, and the Shofar, or Ram's Horn also used in Jewish rites. The last two papers are exceptionally interesting contributions to the significance of music in ritual. The whole book is a clear and capable presentation of accepted psycho-analytical interpretations. It sets forth a large number of interesting facts, and is never obscure even in difficult points of interpretation. The translation is very readable. Prof. Freud contributes a short Introduction.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AT 30 SEPTEMBER 1931

Revenue Account for the year ended 30 *September* 1931

30 Sept. 1930 £ *s.* *d.*	EXPENDITURE	£ *s.* *d.*	£ *s.* *d.*
50 0 0	*To* Rent		57 10 0
73 16 10	Printing		77 19 0
26 13 6	Hire of Halls...		48 8 0
	Office and General Expenses:		
	Secretary	200 0 0	
	Addressing	24 0 6	
	Stationery...	11 2 1	
	Postages and Telephone ...	49 1 11	
	Travelling Expenses ...	5 0 6	
	Subscriptions to outside Associations	6 16 6	
	Audit Fee	10 10 0	
	Sundry Expenses ...	11 13 1	
298 6 1			318 4 7
	Journals Publication Expenses:		
	General Journal	88 7 11	
	Medical Journal	164 13 1	
	Educational Journal ...	02 10 0	
229 14 0			345 11 0
8 13 5	Teas		7 14 1
7 18 1	Scottish Branch Expenses		5 11 4
5 0	Midland Branch Expenses		3 4 0
8 7	Bank Charges		17 10
30 0 0	Committee for Research in Education ...		– – –
46 2 10	Library Depreciation		62 9 3
– – –	Register of Members		41 14 11
– – –	Legal Expenses, *re* formation of the British Journal of Educational Psychology, Ltd.		41 4 0
– – –	Difference on redemption of £1000, 4 % War Loan 1929–42		13 5 6
771 18 4			1023 14 3
386 13 3	Balance, being Surplus of Income over Expenditure		190 11 4
£1158 11 7			£1214 5 7

30 Sept. 1930 £ *s.* *d.*	INCOME	£ *s.* *d.*	£ *s.* *d.*
	By Subscriptions 1931	1317 2 0	
	Less, Resignations and Removals	222 2 0	
1058 1 0			1095 0 0
5 0	Midland Branch (2 Associate Members) ...		15 0
12 12 9	Interest on Deposit Account		3 13 5
87 12 10	Interest on Investments		114 17 2
£1158 11 7			£1214 5 7

Balance Sheet 30 *September* 1931

30 Sept. 1930 £ s. d.	LIABILITIES	£ s. d.	£ s. d.
	Sundry Creditor		
71 14 2	Birmingham Branch		2 9 0
30 17 0	*Subscriptions in Advance*		38 12 0
	Publications Reserve		
	Balance per last Balance Sheet	122 0 0	
	Less, Grant made during year	50 0 0	
122 0 0			72 0 0
	Revenue Account		
	Balance per last Balance Sheet	3810 13 4	
	Add, Surplus of year... ...	190 11 4	
3810 13 4			4001 4 8
£4035 4 6			£4114 5 8

30 Sept. 1930 £ s. d.	ASSETS	£ s. d.	£ s. d.
	Cash at Barclays Bank Ltd.		
	Deposit Account	250 0 0	
	Current Account	4 18 11	
463 5 5			254 18 11
3 4 5	*Postage Stamps in hand*... ...	... ...	8 13 11
	Investments, at Cost		
	£100, New South Wales 5½ % Loan 1924–34	100 0 0	
	£1100, 5 % War Loan 1929–47	1117 6 9	
	£1054, 1*s*. 3*d*. Inscribed War Loan 1929–47	1100 0 0	
	£300, 5 % Conversion Loan 1944–64	318 9 9	
		2635 16 6	
	Note. Market Value of above Securities at 30 Sept. 1931 £2379. 10*s*. 6*d*.		
	500 National Savings Certificates (dated 20 Feb. 1923)	400 0 0	
2949 2 0			3035 16 6
204 7 0	*Subscriptions in Arrear*... ...	... ...	202 13 0
	Library		
	Balance per last Balance Sheet	415 5 8	
	Add, Expenditure during year	209 6 11	
		624 12 7	
	Less, Depreciation	62 9 3	
415 5 8			562 3 4
	Shares in the British Journal of Educational Psychology, Ltd., at Cost	... ...	
— — —			50 0 0
£4035 4 6			£4114 5 8

Note. Stock of Journals. There exists a number of past and present volumes of Journals in Stock, but owing to the possibility that a large proportion of these may never be sold, no valuation can be placed on such stock.

I beg to report that I have examined the foregoing Accounts with the books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shewn by the Books of the Society. The Securities appearing under the heading Investments have been verified by production of the Bankers' Certificate.

T. BARRETT,
Incorporated Accountant.

63, EASTCOTE LANE,
SOUTH HARROW.
10 *November* 1931.

S. J. F. PHILPOTT,
Hon. Treasurer.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

June 27, 1931.	Papers on "Oscillation." (*a*) "Reversible Perspective and the Effect of Conation." By Miss M. I. Dunsdon, B.A. (*b*) "Oscillation in Sounds of Low Intensity." By Mrs H. W. Oldham, B.Sc. Demonstrations of Apparatus. By Mr D. F. Vincent, B.Sc. (*a*) "A mirror tachistoscope without moving parts." (*b*) "An apparatus for producing sounds of predetermined wave-form."

SECTIONAL MEETINGS.

Medical.

June 24, 1931.	Symposium: "On Phobias." By Dr A. R. Redfern, Dr Sybille Yates, Dr James C. Young. The discussion was opened by Dr Emanuel Miller.

Education.

June 1, 1931.	"Herbart and Educational Reconstruction." By Dr F. H. Hayward.
October 5, 1931.	"Vocational Guidance and Education." By Mr H. A. Simmonds, M.A.
November 2, 1931.	"Research of Specific Disability in Spelling." By Mr F. Schonell.

Industrial.

June 15, 1931.	"The Present Trend of Industrial Psychology in Great Britain," read by Mr D. R. Wilson, M.A. (H.M. Deputy Chief Inspector of Factories).

Aesthetics.

July 8, 1931.	"The Grotesque in Art." By Mr Edmund J. Sullivan, R.W.S., R.E.

Joint Meetings.

October 28, 1931.	Papers: "Accidents." By Prof. Major Greenwood, Mr Eric Farmer, Dr Georg Groddeck.

VOLUME XXII APRIL, 1932 PART 4

SOME EXPERIMENTS ON THE ABSTRACTION OF FORM AND COLOUR.

PART II. RORSCHACH TESTS.

BY O. A. OESER.

(*From the Cambridge Psychological Laboratory.*)

I. *Introduction* (pp. 287–288).
II. *Method* (pp. 288–301).
III. *Summary of results* (pp. 301–308).
IV. *General summary and conclusions* (pp. 308–322).
  (*a*) *Colour and form-dominance tests and Rorschach tests* (pp. 308–311).
  (*b*) *General observations* (pp. 311–313).
  (*c*) *Further experimental indications* (pp. 313–316).
  (*d*) *Ontotropic and philotropic selective principles; attention, attitude, perception* (pp. 316–321).
  (*e*) *Suggestions for method in experimental procedure* (pp. 321–322).
V. *Summary* (pp. 322–323).

I. INTRODUCTION.

THE investigations described in the first part[1] showed that the form and colour functions in perception seem, in some individuals, to act independently of one another. Since they act selectively in all persons sometimes, and in some persons all the time, however the experimental conditions are varied, it seems as though the predilection for either form or colour is something inherent in the psycho-physiological structure. It ought, therefore, to show itself at higher levels as well, for instance, in the preponderance of either form or colour memories, in the way in which perceptual material is interpreted, and so on.

It was decided to use some form of the 'blot tests,' which have so often been found useful in experiments on perceiving and imaging, for here the subject is absolutely free to react in whatever way suits him most. He can take as long as he likes and allow his imagination free rein, whereas in the other series he was very considerably restricted. The only carefully standardized series of blots is that published by Hermann Rorschach in *Psychodiagnostik* (Ernst Bircher, Bern and Leipzig, 1921). His series consists of ten symmetrical blots which he chose after exhaustive trials from over a hundred as being most suitable. They are printed on semi-glossy sheets, 7 × 10 in. Five are in shades of grey and black, five are coloured, and they vary in complexity. Rorschach enumerates certain criteria that have to be fulfilled if a series like this is to be of use as a test. In the first place the forms have to be relatively simple, since complicated ones make the scoring too difficult and uncertain. Secondly, they have to obey certain laws of 'space rhythm,' as the plates otherwise lack pictorial quality, with the result that many subjects reject them as 'simply blots,' without attempting to interpret them. Thirdly, they have to be symmetrical. This quality gives the forms the necessary rhythmical distribution on the plates, creates equal conditions for right and left-handed people, and seems to facilitate the reactions of the class of subjects that suffers from too many doubts and inhibitions. A defect—and, from our experience with normal subjects, a serious defect—is that the symmetrical arrangement tends to make the answers somewhat stereotyped. We found that the monotonous symmetry affects some subjects more than others. Most of the form-dominant (Fd) subjects did not seem to mind it very much—their answers would be stereotyped however exciting the blots—but the colour-dominant (Cd) types, and those among the Fd's who belong to the more integrate types of personality, often got so annoyed at the recurrent symmetry that they had to be given half the series on another day. This type of subject hates anything schematized, which forces him into repeating himself too often. If this is realized and care is taken to classify one's subjects beforehand, bad results that do not reflect the personality adequately can be avoided. Simply giving everyone half the series at one session and the other half some other time does not get over the difficulty, since experiments showed that the 'disintegrate,' Fd types did better in one session than in two, partly because they take some time to get 'warmed up,' partly because they feel that they are "not too good at this imaginative stuff," and are not keen on having to go through the experiment twice.

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[1] *Brit. J. Psychol.* 1932, XXII, 200.

II. Method.

The subjects were given the following preliminary instructions: "You will be given a number of blots to interpret in any way you choose and in your own time. The blots don't mean anything in themselves, but it is possible to see almost anything in them, just as you can see faces and figures in the fire or in clouds. I am going to note the average time, so don't uncover a blot until I say 'now.'" The subject was then given the plate, told he could take as long as he liked and say what he liked. There was no point in imposing a definite time limit, since some subjects are much slower than others and are differently affected by a restriction of any kind; also the main object was to have as few restrictions as possible. The reason for the one rule imposed—that they should not begin until the experimenter said 'now'—was the following. It had been noticed in the previous experiments that the Cd subjects easily forgot the instructions, or even that they were supposed to be taking part in an experiment, partly because they were distracted by a rush of extraneous associations, and partly because they were so interested that they wanted to get on to the next slide. The Fd subjects, with a few exceptions that will be separately discussed later, kept up a steady, detached attitude, accurately complying with all the instructions. So we wanted to see whether in the blot series the same sort of thing would happen.

Since Rorschach's book is not available in English, we reprint below the "conditions imposed by each plate" (*Psychodiagnostik*, p. 43):

Pl. I. Black. Failure practically never occurs. Form and movement answers equally easy. The plate is easily interpreted as a whole as well as in details, and contains a number of clear small details.

Pl. II. Black and red. Arouses kinaesthesias more easily than I. Contains an obvious 'empty detail' (*i.e.* a white space between the black and red parts, which has some definite form). Contains one colour, red shading into black, which may already give indications of 'colour shock.'

Pl. III. Black and red. Arouses kinaesthesias most easily. Red and black portions separate.

Pl. IV. Black. Answers about form and movement made relatively difficult. More difficult to interpret as a whole than in parts. The figure is generally admired, but regarded as hard to interpret.

Pl. V. Black. Easiest form, invariably interpreted as 'bat' or 'moth.' Schizophrenic patients, however, often fail, or see moving people in it.

Pl. VI. Black. Generally regarded as the most difficult.

Pl. VII. Black. The important thing here is the white 'empty detail,' which fairly clearly represents an oil lamp. The plate is in a sense complementary to Pl. V, since normal subjects rarely see the lamp, whereas it is frequently seen by schizophrenes.

Pl. VIII. Coloured. Harmonious as regards both form and colour. Gives 'colour shock' to neurotics. Easily interpretable details.

Pl. IX. Coloured. Non-harmonic in colour and form. Easily arouses kinaesthesias. A definite 'empty detail.'

Pl. X. Coloured. Disjointed blots. The interpretation of this plate as a whole is almost impossible.

Our records were examined from the following points of view:

(*a*) The number of answers.
(*b*) The time taken.
(*c*) Is the picture interpreted as a whole or in parts?
(*d*) What is seen, animals, human beings, etc.?
(*e*) How many 'original' answers are there?
(*f*) Is the answer determined by
    (i) Form alone, or colour alone?
    (ii) Form and colour together?
    (iii) Are the forms seen good, or merely vague?
    (iv) What other factors, such as associations, are involved in the answer?
    (v) Do kinaesthesias play a part?

METHOD OF SCORING.

The method of scoring the records can best be seen from the two sample records reproduced below. The first is by a Fd subject, the second by a Cd subject. Each answer is scored according to the mode (whole or part, form or colour), and the content (animal, human, etc.). The following abbreviations are used throughout:

W: The plate is interpreted as a whole.

D: A normal detail of the blot (interpretation by parts).

Dd: An unusually small fragment.

eD: Not the black or the coloured parts, but one of the white spaces between them.

oD: 'Oligophrene detail.' Where the plate readily lends itself to interpretation as a whole, only a small part is taken, *e.g.* in Pl. III only the heads of the men.

F: 'Form answer,' determined by the shape of the blot only.

FC: Both form and colour have determined the interpretation. Thus the pink side-pieces in Pl. IX definitely look like animals of some sort. If the answer is "an animal," it is a form answer; if it is "this must be a fox, because it's red," or, "a pink chamaeleon; it's feet are getting blue because it is climbing on blue ground," the answer is obviously taking both the form and the colour into account.

CF: Here the colour is the primary factor, the form secondary or incidental. Thus the red centrepiece in Pl. III is sometimes called "a bow tie," sometimes "the flames of two candles held close together," and sometimes "a splash of blood." The first would be an *F* answer, the second *CF*, the last *C*.

C: When only the colour influences the interpretation.

H: Human being.

Hd: Part of the human body.

A: Animal.

Ad: Part of an animal.

Ob: An object.

Ori: An original answer.

Lands: Landscape.

M: Motion is seen or experienced in the picture.

Anat: Anatomical detail. This has been scored separately, because it is a very frequent interpretation and quite often gives some measure of the stereotypy of the subject.

F –: Sometimes an interpretation is rather far-fetched, and has very little connection with the actual form of the blot. Such answers are scored *F* –, and the relation between them and ordinary *F* answers can then be studied.

RECORDS.

Subject 6. F *group*. 80 *per cent. form dominance in tachistoscopic series.*

Plate	Introspections	Mode	Content	Time min.
I	Extraordinary moth	*W F*	*A*	
	(Inverted.) German eagle	*W F*–	*Ob*	
	Two people's heads with quavers coming out of their mouths	*D F M*	*Hd*	2
II	Section of the spinal cord	*W F* – *C*	*Anat*	
	Space like a top	*D F*	*Ob*	
	Hands pointing, with index fingers up	*D F M*	*Hd*	
	(Red.) Bad slide of an insect's head	*D FC*	*Ad*	
	(Black.) Like a map of Australia	*D F*	*Map*	
	(Side.) Witch's nutcracker nose and jaws	*W F*	*Hd*	
	(Inverted top.) Man with two Scotch caps	*D F*	*Hd*	2¼
III	Two women in tights with high-heeled shoes, pulling something	*W F M*	*H*	
	(Red.) Poodles with long tails	*D F*	*A*	
	Section of the grey matter in the spinal cord (red centre)	*D F*	*Anat*	
	(Inverted.) Crab	*D F*	*A*	
	Fly's head with big eyes	*D F*	*Ad*	
	(Red, centre.) Bow tie	*D F*	*Ob*	
	Crumpled white waistcoat	*eD F*	*Ob Ori*	3
IV	Two depressed penguins, with their backs to each other, pushing	*W F* – *M*	*A*	
	(Side.) Insect's head with large eyes	*D F*	*Ad*	
	(Inverted.) Bat	*W F*–	*A*	
	(Centre.) Something like a cat with horns in front of a fire	*D F*	*A*	
	(Side.) An old woman, with a bonnet, a bunch of flowers, and a cape	*D F*	*H*	
	(White patches.) Men with goggles on	*eD F*	*H*	
	(White patches, inverted.) Two faces with moustaches	*eD F*	*Hd*	
	(Side.) Might be an old man with a long beard and a bowler hat	*D F*	*H*	
	Shadowy face of a woman in the interior	*Dd F*	*Hd*	
	(At top.) Knuckles with middle finger sticking out	*Dd F*	*Hd*	
	(Bottom.) Might be a policeman	*Dd F*–	*H*	4
V	Is this a bat with a cloven head?	*W F*	*A*	
	(Inverted.) Much more like a bat with ears!	*W F*	*A*	
	(Side.) Man with retroussé chin, long nose and moustache	*D F*	*H*	
	(Side.) A pair of nutcrackers	*D F*	*Ob*	
	Baby bird with open beak	*D F*	*A*	
	(Side top.) Vase	*D F*	*Ob*	
	(Inverted, top) Might be a flat-faced Chinaman with a pigtail	*D F*	*Hd*	
	(This is so obviously a bat that it's hard to see anything else in it)			4
VI	Cyst stage of tapeworm	*W F*–	*A*	
	A curious orchid	*D F*	*Ob*	
	(I can't think why this thing has wings.)			
	(Inverted, top.) A set of mandibles and antennae	*D F*	*Ad*	
	(Side.) Monoliths	*D F*	*Ob Ori*	
	(Inverted.) Like a duck sitting on a nest	*D F*	*A Ori*	
	(Side.) A 'geological hill,' with the strata neatly laid out	*D F*	*Map*	
	A double set of wings, but they don't fit the beast they are attached to	*D F*	*Ad*	2½

Plate	Introspections	Mode	Content	Time min.
VII	Two furious women having a fight; they are hung up by their hair; sticking out their jaws and waving their arms about violently	*W F M*	*H*	
	(White, centre.) Rocking stone	*eD F M*	*Ob Ori*	
	(Inverted.) Might conceivably be an insect's head	*D F*	*Ad*	
	(White.) Globe of a lamp with a fancy brass part	*eD F*	*Ob Ori*	
	Legs of men in breeches; they're running	*D F M*	*Hd*	
	Dog's head raised to howl	*D F M*	*Ad*	3
VIII	This is some very good staining	*W C*	*Ob*	
	(Pink.) Some queer animal	*D F*	*A*	
	Insect's head	*D F*	*Ad*	
	(Top, grey.) A mountain in a Japanese picture	*D F*	*Lands*	
	(Centre line.) Hour glass	*D F*	*Ob Ori*	
	Another, bigger one	*D F*	*Ob*	
	Two rough faces	*D F*	*Hd*	
	(White.) Backbone and ribs	*eD F*	*Anat*	
	(Blue.) Flags	*D FC*	*Ob*	
	(White.) Santa Claus with beard	*eD F*	*H*	
	(White, inverted.) Cornucopia	*eD F*	*Ob Ori*	
	(Small grey.) Fine birds flapping their wings	*Dd F M*	*A*	
	(Top, grey.) A shrimp with stalk eyes	*D F*	*A*	4
IX	(Green.) Old man with a hat, drinking	*D F M*	*H*	
	(Orange.) Two gnomes with peaked caps wagging their fingers at each other; they are kneeling down	*D F M*	*H*	
	(Inverted, in green, top.) Shadow of a hand on the wall	*Dd F*	*Hd*	
	(Centre.) Snake stretched out	*D F*	*A*	
	(Side, green.) A wave curling over on to yellow sand	*D FC*	*Ob*	
	(Inverted, side, pink.) Chubby child's head	*D F*	*Hd*	
	Hood of a cobra	*D F*	*Ad Ori*	
	Lamp in a chimney	*D F–*	*Ob*	
	(Green.) Man crouching	*D F M*	*H*	4
X	Spiders	*D F*	*A*	
	Prince of Wales's feathers	*D F*	*Ob*	
	Mistletoe	*D F*	*Ob*	
	White blood corpuscles of the polymorphic type	*D F*	*Ob*	
	Crab	*D F*	*A*	
	(Pink.) Sea horses	*D F*	*A*	
	Two men in black turbans resting their chins in the blue things	*D F–*	*H*	
	(Inverted, blue.) Head of the dog-faced god	*D F*	*Ob Ori*	
	Snakes sitting on their tails	*D F*	*A*	
	(Blue.) Pelvis	*D F*	*Anat*	
	"Ragged Robin" flowers	*D FC*	*Plant*	
	(Black.) Bladders on sea-weed	*D F*	*Plant*	
	Black-headed canary (yellow) sitting on a branch (blue)	*D FC*	*A*	
	Woolly dog	*D F–*	*A*	
	(Yellow, side.) Cyclops	*D F–*	*H Ori*	5

General observations. At the fourth plate the subject forgot the instructions, and turned over before the experimenter had given the signal; but they were remembered again after the fifth, without a reminder. It must already be obvious to the reader that the subject interprets primarily on the basis of form. The quantitative analysis, which follows below, shows that 92 per cent. of the answers were form answers. When the colour is noted as well it seems more in the nature of an afterthought. Thus even in Pl. X, which consists almost entirely of brightly coloured blobs, the pink shapes are 'sea horses,' the blue is a 'pelvis,' and the 'black-headed canary' is sitting on a bright blue 'branch.' There are not very many original interpretations, and they are none of them particularly striking. We asked a number of the subjects to write a short letter, saying what they thought of the experiment. We quote some extracts from this subject's reply; "The experiment struck me as fairly reasonable, and I tried very hard to be honest. I found it easier to see things in the black blots than in the coloured ones because in the latter the colour often contradicted the shape...." What the subject means by 'being honest,' is that she tried hard to see something. But her remarks were nearly all prefaced by "it might be," or "if this were drawn better, it could be," or "this looks like..., but that part doesn't fit in." Most of her remarks were concerned with the "things that didn't fit in." Her whole attitude, as she herself expresses it, is a 'reasonable' one. There was no emotional response; she was neither particularly pleased with any of the plates, nor did she show signs of fatigue or boredom. The time taken for each plate did not vary much.

These general observations would apply equally well to most of the subjects that, in the tachistoscopic tests, were Fd. But when we come to the other group, the Cd subjects, the picture is completely different. We shall first reproduce a typical Cd's record, and then place the quantitative analyses of the two cases side by side.

Subject 23. CF *group*, 45·5 *per cent. form dominance.*

Plate	Introspections	Mode	Content	Time min.
I	Vertebra	*W F–*	*Anat*	
	A swallow flying; no, a bird that is not so swift ...	*W F– M*	*A*	
	One image on top of another; black thunderclouds viewed from the mountains in Grahamstown	*W C*	*Lands*	
	(Top.) Two bears who have quarrelled and are facing opposite ways	*D F M*	*A*	
	(Inverted.) Crown or mitre; reminds me of Thomas à Becket	*W F–*	*Ob Ori*	
	The holes look like windows; they look untidy ...	*eD F*	*Ob*	3
II	Segment of the spinal column	*W FC*	*Anat*	
	(Inverted.) Beetle with feelers	*W F*	*A*	
	A pair of rabbits sitting up	*D F*	*A*	
	Stag's head	*D F*	*Ad*	
	(Side.) A streaky painting, veiny, almost like a butterfly's wing	*D C*	*Ad*	
	Two faces facing opposite ways	*D F*	*Hd*	
	There's a feeling of motion in it, it's swishing up and down	*M*		
	Rabbit's ears	*D F*	*Ad*	
	A volcano with flames and smoke, like the one I saw at the flick	*W CF*	*Lands*	
	(Red, side.) Grass on fire	*D C*	*Lands*	
	The black seems to have been put on to hide something, but the red is still poking through. That is a red butterfly	*D FC*	*A*	5

Plate	Introspections	Mode	Content	Time min.
III	People pulling very hard; no, my next impression is that they are not pulling hard, but posing, because they are not looking what they are doing	*W F M*	*H*	
	They are holding their heads very erect. It seems to be stencilled on, because of the gaps in the middle. The portion in the centre conveys the impression of going round like a top	*D F M*	*Ob*	
	The red part looks like embryos	*D FC*	*Anat*	
	(Inverted.) Two Dassies	*D F*	*A*	
	High-heeled Russian boots	*D F*	*Ob Ori*	
	The whole thing gives me the impression of dampness, because the colour is that of clouds, big black ones	*W C*	*Clouds*	
	Butterfly	*D F*	*A*	
	A conventionalized Kafferboom	*D F−*	*Ob Ori*	
	(Centre streaked part.) Clouds after rain, beginning to clear up	*D CF*	*Clouds*	
	(Inverted.) The whole looks like one of those vases with short legs, associated with aspidistras	*W F−*	*Ob Ori*	
	Cock stretching himself and crowing	*D F M*	*A*	
	Nothing is clear. It has been smudged while damp. The whole thing is frightfully artificial because it's so symmetrical. Stilted, constricted (that's why those people don't seem to be pulling hard). There is a feeling of strain. The curves are artificial—except the top in the middle.			9
IV	A great black spider	*W F−*	*A*	
	Or a gorilla with humpy shoulders	*W F*	*A*	
	Now it looks like a man with enormous feet trying to dance	*W F M*	*H*	
	(Top.) Beetle's head with feelers	*D F*	*Ad*	
	Beetle	*D F*	*A*	
	Bats wings	*D F*	*Ad*	
	(Shaded parts.) Pressed dead leaves and bits of moss	*D CF*	*Moss*	
	Ringhals sitting up, just about to strike	*D F M*	*A*	
	Gives me a sense of fire	*W C M*	*Fire*	
	Formalized, as though something natural like a beetle had been worked into a pattern or design; symmetrical. The shading strikes me; it has been more carefully done. There is no sense of motion at all. There is this sense of fire: the upward strokes are the flames, with the smoke going off in clouds. A sacrifice in the middle. I get a feeling of furriness like a velour hat. The symmetry of all these strikes me, but this one is less formal than the others, which seem a little artificial. The two halves worry me: it feels like an echo, and that makes it sound sillier than ever, so many far-fetched comparisons.			
	(Top.) Microscopic view of fly's head	*D F*	*Ad*	
	A boot, kicking	*D F M*	*Ob*	
	Map of Italy, without Sicily	*D F−*	*Map*	7
V	Moth or bat. There is a sense of motion; it is flying forwards	*WF M*	*A*	
	(Side.) Crocodiles	*D F*	*A*	
	Two ringhalses spitting at each other	*D F M*	*A*	
	A jackrabbit sitting up	*D F M*	*A*	
	The nobbly edges remind me of barrows	*D F*	*Ob Ori*	
	There's a sense of depth in this blackness; like coming home from a dance last vac. You're always penetrating into the blackness. You want to drop into it like cushions.			

Plate	Introspections	Mode	Content	Time min.
	(Small projections, top.) The Amadoda mountains	*Dd F*	*Lands*	
	Strong impression of something flying at speed	*M*		
	Feeling of octopus; if you slid into it, it would close up at once	*W F – M*	*A*	
	The gargoyles of Caius	*D F*	*Ob Ori*	
VI	A dragonfly with a too fat head, on top	*D F*	*A*	
	North American leatherwork, totem poles, same shading, tattooing, and colouring; height, but no grace	*W F*	*Ob Ori*	
	Vague impression of a beetle	*W F –*	*A*	
	Chinese banner	*W F*	*Ob Ori*	
	I like the soft greys; they are different to the greys in the clouds, because there are darker spots in them.			
	River bed with pebbles	*D F*	*Lands*	
	Feeling of motion in the top right-hand bit, not in the rest	*M*		
	Body of a butterfly with feathers	*D F*	*Ad*	
	Too symmetrical for a natural object			
	Smoke rising from a kraal at night; that connection almost gives you the sense of smell of dung fires. Smoke barrage. The black spots make it artificial barrage rather than a fire. There is an unpleasant suggestion in the whole thing of blood-sucking spiders, horrible squishy things I read about as a child.	*W C F*	*Lands*	
	Ovary of flower (Eridaceae), seeds just beginning to form	*D F*	*Plant*	
	A staff with bells, used in Morris dancing	*D F –*	*Ob Ori*	
	(Edge.) Map of sea coast	*D F*	*Map*	7
VII	Oh! Two little girls looking at each other; frightfully artificial, not a comfortable position. I feel the thing might move, because it is on rockers	*W F M*	*H*	
	Stag's horns sawn across the middle and mounted on a hinge	*D F*	*Ad*	
	Cut yews at Hampton Court, hideous	*D F*	*Plant Ori*	
	An ornament difficult to dust	*D F*	*Ob*	
	Something like one of the sections of the vertebral column	*D F*	*Anat*	
	Those kids have rouged, but in the wrong place, on the chin. It is a Victorian thing, badly knit together. They slouch. I don't like it			4 5
VIII	What a relief to get some colours! Pink chamaeleons; their front paws are getting blue because they are climbing on the blue ground	*D FC*	*A Ori*	
	A Swiss mountain in snow; the prickly edges in grey are pines; the blue is glaciers	*D FC*	*Lands Ori*	
	I don't like this red, brown and pink, that's a squashed butterfly	*D CF*	*A*	
	One chamaeleon is badly drawn, it has a languorous eye; the other one is fierce, it has been fighting; the one feels the climbing of the mountain to be a joke, the other takes it seriously.			
	Brisk flames; but together with the blue sky it gives you a sense of coldness	*D C*	*Fire*	
	(Bright green spot.) Woolworth jewellery	*D CF*	*Ob Ori*	
	(Inverted.) Antbear	*D F*	*A*	
	Flowing water from jars, and congealed milk	*D C*	*Ob Ori*	5
IX	I like the colours, only the pink worries me. I have a sense of something about to emerge.			
	Two old men, Santa Clauses, with whips, red noses, and a bottle in either hand, scrapping in a jovial fashion	*D FC M*	*H Ori*	
	Two fiddlers dancing to their own fiddling	*D F M*	*H*	

Plate	Introspections	Mode	Content	Time min.
	An elephant with a grin, climbing a pole that is about to crack	*D F– M*	*A*	
	Water down below there	*D C*	*Ob*	
	Fishes	*D F*	*A*	
	Mossy logs	*D CF*	*Ob*	
	Bit of the spinal column	*D F*	*Anat*	3
X	Flowers	*D CF*	*Plant*	
	Spiders and beetles having a dance	*D F M*	*A*	
	Blue spider waving a bunch of grass	*D FC M*	*A*	
	Black beetles doing a pyramid with poles	*D F*	*A*	
	(Red.) Frog	*D F*	*A*	
	Black beetles, flying	*D F M*	*A*	
	The whole thing is a grand pyramid, every animal supporting every other	*W F M*	*A Ori*	
	A horseshoe made of evergreen	*D FC*	*Ob*	
	Pink flames in an artificial picture	*D C*	*Flame*	
	(Yellow.) Someone has been playing about with a paintbox	*D C*	*Ob*	
	There's a jovial sense in the whole thing; the spiders are positively prancing, like Felix; here are some more jolly black flies	*D F M*	*A*	
	Yellow Poms barking	*D FC M*	*A*	
	A totem pole being supported by the whole lot	*D F*	*Ob Ori*	
	A brown fish	*D FC*	*A*	
	The whole thing looks very much like the wallpaper in a boarding-house bedroom, very unrestful	*W CF*	*Ob Ori*	5

General observations. The total number of answers in the first record is 85, in the second, 92. The two can therefore be compared directly as regards the number of answers about form, colour, etc. The first point to be noticed is the frequency with which subject 23 interprets the picture as a whole: there are 23 such answers, compared with 12 in the case of subject 6. Secondly, we find a great difference in the number of answers determined by form alone. The first subject scores 92 per cent., the second 66 per cent. If we take only those answers into consideration in which the forms are 'good' (*i.e.* the $F+$ answers), we find 81 and 55 per cent. respectively. Thirdly, there is a great difference in the number of kinaesthetic and synaesthetic elements. The latter are difficult to assess; but for the former we find 11 'M' answers as compared with 28. The Fd subject has scored 11 'original' interpretations, the Cd 13. In themselves these numbers are not significant; but it is difficult to decide what constitutes an 'original' answer. If we had followed Rorschach's criterion, that an answer is original if it is given once in a hundred experiments, we should have to count many more for subject 23, *e.g.* "the Amadoda mountains near Grahamstown," "smoke rising from a kaffir kraal, and the smell of dung fires," "the feeling of furriness, like a velour hat," etc. No one who reads these two records will hesitate to call the second one the more original.

This record shows us a completely different type of reaction. Where the first subject was detached and 'reasonable,' the second is far more personal, subjective, and 'unreasonable.' The associations that are called up all have a peculiarly personal touch, and so the record is full of "I feel," "I don't like this," "this worries me," "this reminds me," "Oh! this is nice!" and many more which have not been reproduced so as not to make the record unduly long. The attitudes of the two subjects, again, were entirely different during the sessions. The Cd was continually laughing, wriggling about, shuddering at the "squishy spiders," going off into long monologues about former experiences, in short, thoroughly enjoying herself.

One further observation, to which we shall have occasion to return later in greater detail, concerns the 'effort after meaning.' The Fd is characterized by real effort. For him, most of the forms are pretty well meaningless, since they do not "correspond to nature," as he says quite frequently. He has to search and rummage about among his images to find something that will correspond to the blot; and he is very rarely fully satisfied with his interpretation. In the Cd, however, this effort is hardly apparent. The succession of images is so swift, that it has the appearance of perfect spontaneity. One often has the impression that as soon as the Cd has given a name to the blot, it immediately becomes that thing, and he can go on almost indefinitely adding secondary details. He is surprised if the observer sometimes cannot see the resemblance. We shall come across examples in which this recreating of the perceptual material is much more striking, but at this point shall content ourselves with a comparison taken from the two records quoted in full above. On Pl. VIII there are two pink side-pieces that look like some animal with a long tail. For all the Fd's this is merely "some kind of animal," or, in the first record above, "some queer animal." But the Cd's invariably weave a tale about them, and fit them into the rest of the picture, making them climb up rocks, or fish in the stream (the blue side bits). "Pink chamaeleons; their feet are getting blue because they are climbing on the blue ground...one chamaeleon is badly drawn, it has a languorous eye; the other is fierce, it has been fighting; the one feels the climbing of the mountain to be a joke, the other takes it seriously."

It must be emphasized that the kinaesthetic experiences are quite real and vivid. The motions are actually experienced by the subjects. Often they wave their arms about, or take up curious contorted positions on the chair, to show "what it really feels like." In every way this subject is intimately concerned with the material, lives in it, and takes up an attitude towards it that is in many ways the direct opposite to that taken up by the first subject. It is this close personal contact with a situation that Jaensch calls *Kohärenz mit der Aussenwelt.* He compares the consciousness of the integrate with an empty space, in which objects meet somewhat indiscriminately, whereas the disintegrate has numerous firm systems already present in his mind. His attitude is determined by them, and not so much by passing moods or the outside world.

This is evident, too, in the way in which instructions are followed. In the case of the Fd subjects, *one* instruction not to turn over the cover until the observer says 'go,' is sufficient to set up a permanent attitude. Perceptions and subjective sentiments are split apart, and the latter do not interfere with the former. Here, as in most other situations, whether in the psychological laboratory or in real life, there is a permanent attitude (*Einstellung*) towards objective fulfilment of duty, and an instruction in an experiment becomes part of this general principle of active duty. But in the case of our Cd subjects, an instruction only sets up a temporary or momentary attitude. In the excitement of the moment this is forgotten, because the subject cannot be said to have a permanent attitude. He is always dependent for his responses on the immediately present general situation. So we find in our Cd subjects that after the first few plates the instruction is forgotten. The subject snatches at the next plate and goes on with his description. If the instruction is repeated, it may have a permanent effect, but sometimes three or four repetitions are of no avail. Even if the observer, by holding on to the plate and repeating his instruction, manages to impress the Cd subject so that he waits for the command, his attitude is different to that of the Fd. He turns towards the observer and eagerly or impatiently waits for the signal; the Fd, if he does look at the observer, is cool and detached, and hardly lifts even an enquiring eyebrow. 'Coherence', or 'cohesion', then, is an easily observable fact of great diagnostic value. The following is an example of the notes made in the case of a Cd (the numbers refer to the plates): "II, instruction gone; III, instruction repeated; IV, instruction gone again; V, instruction repeated. I held on to the plate, made a distinct pause before telling her to go carry on, and gave her the plate myself; VI, instruction gone again."

Wiedling has studied the distractibility of subjects of the integrate and disintegrate types[1]. They were asked to answer either 'yes' or 'no' according as a certain form was or was not present on a tachistoscopically exposed card. In one series the form was a triangle. At some stage, *e.g.* the tenth card, there appeared a card containing a number of non-geometrical figures, or a caricature of a chicken. Instead of saying 'no,' the integrates—among whom were a number of subjects who had read 'synthetically' in my reading experiments[1]—laughed and described the card. Frequently one such card was quite sufficient to break down the carefully built up response habit 'yes' or 'no.' On some cards the triangle was present in a disguised form, *e.g.* ΔeaΔ; but although the word was correctly read, the triangle was not recognized at all. The disintegrates—among them my 'analytic' readers—went on calmly saying 'yes' or 'no,' without effort, and immediately recognized the triangle in the words. They have what Jaensch has called an *Invarianztendenz*, a tendency towards invariability. For them, the external universe is, within fairly wide limits, an invariant, and so is their attitude towards it, whereas this tendency has not yet developed in children or in integrates to anything like the same extent. As against the tendency to act and to perceive invariantly, the perceptions of the integrate are directed towards relations that have a meaning *for himself*. The instruction not to lift the cover has no strong connection with the momentary personality and is 'forgotten.' Similar observations were made in the *C–F* tests. There, of course, the situation was very much simplified and more unequivocal. Only four slides had to be examined before a new main form was shown. Thus the instructions were practically repeated at every fourth slide. Even so it happened once or twice that a subject, after looking at a slide, said "Oh, I quite forgot what I had to do. I merely looked at the coloured slide as a whole. I like these intense colours." Apart from that, their output of answers was a matter of personal pride. They were concerned with what they had *done*, not with *what* they were supposed to do. The Fd's simply said, "I think it was at 9 o'clock; I also saw a triangle at 3, a circle at 5 and something blue at 12." The Cd's said, "Oh! it went much too fast. It was at 9. There were 18 figures, weren't there? Lots of blues and reds and greens. Red zigzag at 6 o'clock...was that right? Why don't you tell me whether I was right or wrong?"

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[1] H. Wiedling, "Experimentell-strukturpsychologische Untersuchungen über das Willensleben, etc." *Grundformen menschlichen Seins*, pp. 343–421, Berlin, 1929.

The following table contrasts the numerical scores of the two subjects whose records were given above:

Subject	*F* % (tachist.)	Time min.	No.	*W* %	*Ori*	*A* %	*F* %	*F*+ %	*F*− *F*+	*C* %	*M* %
6	80	34	85	14	13	34	92	81	11	8	13
23	45	55	92	25	14	45	66	55	11	34	30

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[1] O. Oeser, "Tachistoscopische Leseversuche, etc.," *Zeitsch. f. Psychol.* CXII. 1929, p. 112.

From this table we can add several remarks about our subjects. In the first place, although the number of answers is nearly the same, the times are 34 and 55 min. respectively. This does not mean that subject 23 (Cd) was slower. On the contrary, her talk, compared to that of subject 6 (Fd), was like a waterfall compared to a trickle, so that the observer had difficulty in writing it all down. If we were to count the actual total number of statements made, the figures would be somewhere in the proportion of 4 or 5 to 1. The times merely give us some indication of the amount of 'silent work' put in by the Fd's in order to produce something. The greater number of 'Wholes' (*W*) has already been remarked upon. The equality of *Ori* per cent. is interesting, but will be discussed with respect to the tables as a whole. The difference in the *A* per cent. is not significant, since the tables show that *A* per cent. is practically constant for all the groups (42 per cent.). But in this particular case the figures, 34 and 45 per cent. indicate a greater degree of stereotypy in the Cd subject, which needs explaining, since it to a certain extent seems to contradict what was said before. Probably the explanation can be found from a consideration of the general attitude of the two subjects. The Fd closely scrutinizes each part of the plate and is guided in his final conclusion by small detail. The Cd, on the other hand, looks at a blot and immediately gets an impression, which is talked about and which brings up a lot of associations. Therefore blots that are only vaguely similar on different plates are not scrutinized very carefully. Secondly, the scoring obscures the fact that a greater variety of animals may be seen by the Cd, whilst the Fd, who suffers from perseveration, goes on calling those shapes 'animals,' or 'dog's heads.'

Rorschach considers that the relation of the *C* answers to the *M* answers is indicative of the type of personality. The *C* answers are scored as follows for this purpose: *FC* ½, *CF* 1, *C* 1½. He distinguishes two types, the 'introversive' and the 'extroversive.' They are to all intents and purposes the same as Jung's types. For the first, *M* is greater than *C*. He has a more differentiated intellect, his productivity is dependent on inner factors, his mental life shows a more 'inward' tendency, so that he is less adaptable to reality and emotionally more stable, his rapport is more intensive than extensive, his motor expressions are more stable and controlled, and he is on the whole much more solitary and unpractical. For the second, *M* is less than *C*. This type is much more productive, although his intellect is more stereotyped. His life tends to outward expression, his emotionality is less stable, he is far more adaptable in every way, shows more extensive than intensive rapport, is motorically excitable, gregarious, and practical.

Now if the two subjects are scored in this way we obtain the following figures:

Subject 6: $M : C$, 11 : 4½, or 2·5 : 1.
Subject 23: $M : C$, 28 : 31, or 0·9 : 1.

In other words, subject 6, who is Fd in both the tachistoscopic and the Rorschach series, belongs to the introversive type, and subject 23, who is Cd in both series, to the extroversive type. These figures are therefore in complete accord with our general analysis. As regards the 'gregarious' aspect, we know from personal contact over two years that this conclusion is also true. Subject 23 is very sociable, has innumerable friends and acquaintances, is lively and 'jolly,' and gets on easily with everyone. Subject 6 has fewer friends and a much smaller circle of acquaintances, because she is so 'shut in' and difficult to get to know.

III. SUMMARY OF RESULTS.

We can now examine Tables I–III, Appendix, in which the quantitative results of the Rorschach tests are summarized.

(1) *Time.* The average time taken by all subjects is about 45 min. A comparison of the first two columns of Tables I and IA shows that on the whole the *C* group took longer, and gave somewhat fewer answers per minute. Curiously enough, the average time for women (Table IA) is shorter than it is for men; but the men gave slightly more answers per minute. These times are far from accurate, and were noted more for the eventuality of one or two subjects taking a very long or a very short time. The shortest time, 22 min., was taken by subject 30 (Cd); she gave

Table I.

All subjects.

Group	Time (min.)	No. of Ans.	% *W*	% *Ori*	% *A*
F	39	70	18	12·5	42
FC	41	59	21	8·4	42
CF	53	69	24	9·4	38
C	50	74	32	12	41

Table IA.

Group	Time (min.)	No. of Ans.	% *W*	% *Ori*	% *A*
Men	51	76	23	14	38
	(52)	(77)	(20)	(12)	(38)
Women	42	60	24	9	42

The figures in brackets are the averages without subject R.

40 answers, 38 per cent. of which were *W* answers, and 35 per cent. *C*. The longest times were taken by subjects 28 (*CF* group, female, 91 min.), 19 (*CF* group, male, 87 min.), and 31 (*C* group, female, 87 min.). They gave 136, 132, 103 suggestions respectively, or 1·5, 1·5 and 1·2 per min. The time, therefore, does not show up any characteristic differences. Rorschach states (p. 10) that the average reaction time is 20–30 min. None of our subjects took as little as 20 min., and 24 took over 30 min. This is probably due to the fact that they were all highly intelligent university students or art students, with a large stock of associations.

Table II.

All subjects.

Group	% *F*	% *F*+	(*F*) – (*F*+)	% *C* [*FC*+ *CF*+ *C*]	% *M*
F	91	79	11·5	8	10
FC	84	71	12·2	16	14
CF	79	65	14·6	20	12
C	71	55	15·8	29	13

(2) *The number of answers* does not show up any characteristic differences either. The smallest number was 30 (*CF*), the largest 137 (*C*). The smallest number of *W* were 6 (*F*) and 4 (*FC*); the largest 37 (*F*) and 34 (*C*). As is to be expected, the percentage of wholes falls as the number of answers increases.

(3) *The number of original answers* (*Ori*). Men on the whole seem to give more *Ori* than women, the figures being 14·3 and 8·9 per cent. respectively (Table II A). According to Rorschach, 10–20 per cent. *Ori* indicates medium intelligences, 0–10 per cent. medium practical intelligences, pedants, and people suffering from depressions. Our average *Ori* per cent. varies from 8·4 to 12·5 per cent. (Table I). Perhaps the average intelligence of Cambridge undergraduates is not very high after all! The highest recorded is that of subject R., 46 per cent. He is undoubtedly intellectual as measured by examination results, having obtained firsts in Mathematics, Physics, and the Engineering Tripos. According to Rorschach he should be an artist, as only artists score as

Table II A.

Group	% *F*	% *F*+	(*F*) – (*F*+)	% *C* [*FC*+ *CF*+ *C*]	% *M*
Men	82	70	11	18	13
	(84)	(73)	(11)	(16)	(11)
Women	80	66	15	20	13

The figures in brackets are the averages without subject R.

high as that. But his temperament and his achievements are not in any way artistic. Three practising artists scored 12, 19 and 24 per cent., and those subjects who were instructors and pupils at the Cambridge School of Arts and Crafts scored between 9 and 26 per cent. This shows that many of Rorschach's conclusions can be accepted only conditionally, subject to further experiment. *A priori* one would not think that artists would find these blots particularly interesting, and this was borne out by experience. A low *Ori* score does not seem to be correlated with intelligence either, since among our subjects several who scored 5 per cent. or less obtained firsts and seconds in various triposes. We must conclude, therefore, that Rorschach's Table VII, p. 38, is certainly not valid for normal people as he gives it. Whether or not it is true for psychotics, we have as yet no means of judging.

(4) *Animals* (*A* per cent.). The greatest number of interpretations of one kind is concerned with animals. Rorschach finds that the average for quite normal people lies between 35 and 50 per cent. Our figure, 41 per cent., is thus in good agreement. Individual variation is not as great here as in any other score. Thus the highest is 54 (*CF*), the lowest 27 (*C*), but all the others lie between 30 and 50 per cent. Bartlett found an average of 72 per cent. animal suggestions[1]. This is probably due to the fact that he used smaller, fairly undifferentiated blots, in which it is often impossible to see more than two or three animal caricatures.

Table III.

All subjects.

Group	% Extroversive $M<C$	% Ambiequal (Coartative) $M=C$	% Introversive $M>C$
*F**	14 [0]	14 [0]	72 [100]
FC	36	27	36
CF	82	—	18
C	100	—	—

* The figures in brackets are the percentages without subjects R. and M., who are exceptional in every respect (*vide* text).

(5) *The number of blots interpreted as a whole* (*W* per cent.). The third columns of Tables I and IA show the number of *W* interpretations given. These figures seem to be of some significance, since there is a perfectly steady progression, 18–21–24–32, from the *F* group to the *C* group. Also, if we leave out subject R., who, with 63 per cent. has almost twice

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[1] F. C. Bartlett, "An experimental study of some problems of perceiving and imaging," *J. Brit. Psychol.* 1916, VIII, 222.

as many as the next best, the men average 19·8, the women 24·3. Even if we count R., the women score more than the men. Rorschach states that the score of women is always lower than that of men. He gives the average for men as 5–7 and for women 4–6, and says that more than 7 *W* are seen only by intelligent persons and artists, who have a greater mobility and wealth of associations. Our lowest scores were both made by women, 4 (*FC*) and 6 (*F*); but all the rest lie between 10 and 30. This difference between our results and Rorschach's may be due to our highly selected university material. The reason why he gives lower figures for women than for men may be due to the fact that his normal women all seem to have been housewives, who would naturally possess a smaller stock of associations and, possibly, a less prolonged education. It again shows the need of specifying one's material. In tests of this kind many-sided contact with the world is obviously going to show itself in more numerous interpretations. Our subjects were all of the same intellectual standing and training, so that differences due to type would have a greater chance of showing themselves. The fact that integrates are more inclined to judge these plates as a whole has been commented on before, by Schenck and Jaensch. Moreover, integrates have been found to be Cd and women are more Cd than men. It was shown in the previous section that our women subjects were, on the whole, more Cd. Our figures agree with all these observations, Rorschach's do not.

(6) *The form interpretations.* The majority of interpretations was made on the basis of form alone. Only in the case of subject R., whose score was 51 per cent., did the percentage fall below 65. The form interpretations are scored as *F* + and *F* –. A certain degree of subjectivism is inevitable in scoring an interpretation as 'weak' (*F* –), and no hard and fast rules can be laid down. There are a certain number of forms on each plate that are seen by nearly everyone. These are called *F* +. Others have to be judged on their individual merits. On the whole, experience soon shows whether the form really is vague or far-fetched. One can always question the subjects and so come to a decision. Table II shows the steady increase in *C* per cent. and decrease in *F* per cent. as we go from group *F* to group *C*. Table II A shows that as in the tachistoscopic series, women are more Cd than men. The fourth columns are also interesting. They show that the Cd give more *F* – interpretations than the Fd. Rorschach remarks that depressed moods heighten the number of *F* responses, cheerful moods lessen them and increase the number of *C*. Those subjects who feel the 'work of assimilation' most strongly, *i.e.* those who are most conscious of the fact that they have to make an

effort to get meaning into the blots, give the greatest number of *F* interpretations. There is thus some connection between accurately distinguishing forms and the amount of intrapsychic work that has to be done. For the Cd a weak impression can immediately become a perception, which is strong enough to maintain itself even against glaring incongruities in the objective material. But the Fd always tests every suggestion by reference to the material itself.

Table III A.

Group	Extroversive %	Ambiequal %	Introversive %
Men	62	7	31
Women	53	32	16

(7) *The colour interpretations.* These fall into three groups, (i) those in which the form is the primary determining element (green caterpillars); (ii) those in which colour is the primary factor, but in which form also enters (bunch of flowers in a vase); (iii) those in which colour alone determines the answer (a crushed strawberry). Apparently only epileptics, particularly demented epileptics, *name* the colours, or say, "something red." None of our subjects did this. Only two subjects in the *F* group gave primary *C* answers, one each, and two other subjects gave two and four *CF* answers respectively. All the other *C* answers were *FC*. In the *FC* group of subjects, more colour answers begin to appear, four subjects gave one *CF* and one *C* each, four gave neither. But in the *CF* and *C* groups every subject gave a number of colour and primary colour interpretations. In column 4, Table II, all colour answers have been added together. We see that the fourth group, the Cd; gave nearly four times as many as the first, the Fd.

Rorschach finds among his psychotic patients that all depressives give few, all maniacs many *C* answers. Those patients whose emotional and affective life is relatively stable, such as depressives, pedants, the indolent, stereotyped, and the more serious cases of stereotyped schizophrenia, give few or no colour interpretations. All the more excitable ones, epileptics, organic and mania cases, give many. There is a close parallelism between high *F* per cent. and low *C* per cent. Primary *C* are found only in irritable and abnormally impulsive types of personality, so that primary *C* is a measure of impulsiveness. Now we have definitely found a group of people who give a large number of primary *C* answers. But they cannot be said to be abnormally irritable, impulsive, or manic, nor to have excessively unstable emotions and affects. One explanation for Rorschach's conclusion is that he did not find any normal Cd's among his healthy control subjects in Switzerland. Munz reports that he had to go to Wurttemberg to find pyknic types (who are also Cd), because they are so very rare in Switzerland[1]. So Rorschach quite probably did not come across any, and those who gave many primary *C* may have been of a mixed type, modified by excessive inhibitions and repressions. But on the whole we can agree with Rorschach, provided we remember that the Cd can be a perfectly normal personality type. Most young children, too, are Cd. It is true to say that this type is affectively less stable than the Fd; but this only means that their emotional side is more easily stimulated. The balance is quickly regained again and an emotional disturbance does not affect them as much as it does the Fd types. They are more sensitive in many ways, and their emotionality is strongly egocentric. *FC* interpretations represent, to some extent, the necessity of having to adapt oneself with conscious effort to a situation, to take up a valuing attitude towards it. This the Fd does, whilst the Cd fits easily and naturally into his environment. One sign of the emotional egocentricity of the Cd, in spite of his outwardly directed attitude, is that he tells a story essentially as to how it affected him, without much regard for logical sequence or for the listener.

(8) *Kinaesthetic elements* (*M* per cent.). These are difficult to interpret by the observer. Thus "a man running" is usually not an *M* answer, unless the subject 'feels' the actual strain in himself in some way, or actually sees motion in the picture. The last columns of Tables II and IIA do not show any difference at all. Yet the impression we get from the records is that the more Cd subjects, particularly those in the *CF* group, have more, and more pronounced kinaesthetic images than the *F* groups. The following introspections are all taken from the records of subjects in the *CF* and *C* groups. "Two devil dancers going round at great speed. They make me feel quite giddy. Now the whole picture

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[1] "In spite of the rich material to be found in the Zurich polyclinic, it was difficult to find 20 true pyknics, whilst pronounced schizaffines, particularly leptosomes, were very frequent. This may partly be due to the place at which we made our selection, but above all to the anthropological characteristics of the majority of Swiss. Since our main object was to study the pyknic type, we tried to get records in South Germany, where the pyknic bodily constitution seems to occur most frequently in its pure form. *And indeed it was extremely easy to find perfect examples of the pyknic constitutional type wherever one turned, whereas leptosome types had to be hunted for*" [my italics]. Emil Munz, "Die Reaktion des Pyknikers im Rorschachschen psychodiagnostischen Versuch," *Zeitsch. f. d. ges. Neurol.* 1924, xci, 68.

seems to be whizzing round." "A great many pictures convey animated movement to me." "Lamp hanging down from the ceiling (Pl. II). It doesn't last long. Now I see the animals again. Now the lamp has come through again, with dirty wallpaper behind it. It's magnificent, this; it pushes out when it is a lamp, and recedes to great depth when I see the animals. The illusion of movement is strong. I shall go dotty if I look at this any longer! Now the colours have started violent up and down movements." "A bat with flapping wings, it's eyes are looking straight at me, they have a cruel, inhuman look. It revolts me, I hate looking at it. The wings are slowly moving and vibrating, as though it is ready to pounce."

The kinaesthetic elements can get very strong. They are different in some respects to the kinaesthesias of the *F* and *FC* groups. Motion seen by them is usually felt by the subjects as strain, and may therefore easily be part of what we mean when we speak of the *effort* after meaning. Thus a blot suggests a human being. He is in a curious attitude. The subject tries to find out what the attitude is, and says finally, "a man crouching down beside a precipice, waving his arms, about to fall over." In the case of most Cd's, the movement is seen arising out of the perceptual material itself. The Fd's, on the other hand, *project* the movement into the picture. This point is of some importance, particularly as Schenck[1] has carefully analysed a number of subjects whom he calls the 'dynamic' type. They are greatly affected by seen and imagined movement, *e.g.* in tachistoscopic experiments on the fusion of moving forms. One of his subjects gave 63 per cent. *M* in a Rorschach series. Curiously, Rorschach considers that the more stable the affects are, the more *M* interpretations there are. This can only mean that he scores an *M* only when the subject feels the movement as a strain or tendency within himself. We have therefore scored as nearly as possible in Rorschach's sense, so that a further comparison may be made.

(9) *Relation of* M *to* C. Since both *M* and *C* give us some insight into the affective make-up of personality, a comparison of these two factors will tell us something about the 'experience type' (*Erlebnistypus*), that is, the way in which a person responds to his environment in general. In the very interesting and instructive tables on pp. 65 and 66, Rorschach has summarized his experience with normal and psychotic people. He finds that the more extroverted and adaptable a person is, the more does the number of *C* answers predominate over the number of *M*. The

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[1] H. Schenck, "Experimentell-strukturpsychologische Unters. über den dynamischen Typus," *Zeitsch. Psychol.* 1930, cxiii, 91.

more abstract, introverted, and emotionally stable a person is, the greater is the preponderance of *M* over *C*. As a rough empirical rule he finds that the best picture is given if the *C* answers are scored as follows: *FC* ½, *CF* 1, *C* 1½. The total score for *C* is now compared directly with the number of *M*. In extroverted types, *M* is less than *C*, in introverted types, greater. When *M* is equal to *C* we have an 'ambiequal' type. If both *M* and *C* are numerous in such a type, we have the optimum control of emotional and stabilizing factors; if they are infrequent, the person is pedantic, stiff, an 'I-know-everything-better' type.

Tables III and III A show the proportions among our subjects. Two of them, who fell into the *F* group on the basis of the tachistoscopic experiments, were subsequently found to be exceptional persons in nearly every respect. On all the curves they fall right outside the average for their class. They will be analysed separately later. We see from Table III that if we except these two, the *F* group consists entirely of 'extroversives,' *i.e.* in every case *M* is greater than *C*. Table III A shows the distribution for men and women. There are actually more extroversive men than women, but twice as many introversive men as women. The 'ambiequals,' with one exception (subject R.), are all women and not very different to the extroversives. R., too, is in some respects more extroversive than introversive. If we keep this in mind, the proportion of extroversives in men and women becomes 69 : 88. The three women from the *FC* group, and R., who was the best Fd in the tachistoscopic series are ambiequal. The *CF* group is the most widely scattered, the *F* group least. This confirms our suspicion that the *CF* group consists of mixed types, and further experiments will have to be made to determine them in more detail. It seems certain that the synaesthetics fall into this group, and they are sometimes strongly integrate, at other times strongly disintegrate, depending on their mood and on what particular response they are called upon to make.

IV. GENERAL SUMMARY AND CONCLUSIONS.

(a) *Colour and form-dominance tests and Rorschach tests.*

These investigations were not concerned with the whole problem of fundamental types and the necessary or sufficient criteria for distinguishing them. It does seem clear, however, that the difference between colour and form abstractors, which has been shown up by these investigations, is due not merely to these particular experiments, but has some more fundamental basis and may eventually lead to more far-reaching conclusions. The following considerations may eventually help to clear up this curious phenomenon.

To begin with, there are a number of general observations that have forced themselves on our notice. Taken as a group, the subjects who are Cd in these experiments are a more delicately balanced type. They are easily enthusiastic, and as easily discouraged. Their responses are coloured by emotional outbursts, and they are far more interested in their imaginative processes and the opinions of the observer, than in the correctness of their own observations. Their attention is very fluctuating, and they are more suggestible than the Fd subjects. Put briefly, they evince a far higher degree of subjectivism than do the Fd's. If a Cd is told that his observations are wrong, he is worried, and, if only subconsciously, rather hurt, since he always wants the observer to have a good opinion of him. If an Fd is told he is wrong, he takes it as an objective fact. There is a similar difference between objective and subjective attitudes when the two types are told that they have made good observations. The Fd type is pleased at what he has *achieved*, the Cd type is pleased at himself.

These observations fit in, in a general way, with those made by Lutz and Scholl. They state quite definitely that all their Cd's were cyclothymes, in the sense of Kretschmer's typology, and all their Fd's schizothymes. But in this form we have been unable to verify their generalizations. Indeed, with one clear exception, none of our Cd's were cyclothymes; most of them would fall into the schizothyme class. This merely goes to show that Kretschmer's typology is not universally valid—an objection that has been proven over and over again in the researches of the Marburg Institute. The Kretschmerians are realizing this themselves, too. Thus Kroh[1] found that the schizothymes are not as good as the cyclothymes in tasks that require rapid alternation of attention, whereas Enke and Heising[2], working in Kretschmer's clinic, found exactly the opposite. The purely physical characteristics by which Kretschmer's types are differentiated, are not sufficiently penetrating criteria. But this line of criticism and investigation does lead in one direction that may ultimately be found to be important. Competent observers who have worked in Tübingen, say that Kretschmer's typology is undoubtedly applicable with fairly high accuracy to the population of that region of Germany (Wurttemberg); and since Kretschmer has come to Marburg, he admits himself that the Hessians on the whole do not fit into his typology quite so well. True cyclothymes seem to be extremely rare in England, where schizothyme forms predominate. Munz makes the same statement about Switzerland. Therefore if these colour and form-discrimination tests can be worked out into really satisfactory tests, they might give us a tool for investigating racial types and characteristics. Other investigators have found that children and women are more Cd, and our investigations have confirmed this in the case of women.

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[1] Kroh, "Experim. Beiträge zur Typenkunde," *Zeitsch. Psychol.* Ergänzsbd. XIV, 1929.

[2] Enke u. Heising, *Zeitsch. f. d. ges. Neurol. u. Psychol.* 1929, p. 118.

On the basis of some relevant work on colour and form memory, Said comes to the conclusion that each kind of memory is a specific, unitary function, "in the sense that there is a general factor, 'Cl' (or 'Fr'), which functions generally, and is involved in the process of apprehending and recognizing all colour (or form) groups and combinations, independent of 'G'[1]. Training of colour and form memory is possible to a great extent, but the improvement is specific, or non-transferable. Improvement in one does not affect the other.

The quite general observations made above on attitude and the emotional under-currents of personalities are confirmed by the second series of experiments performed by the same subjects. The first series dealt with unconscious responses. There was no time for reflection and comparison. In the second series the subjects were given a number of blots to interpret at their leisure. Compared to the Fd subjects, the Cd's were invariably more alive, amused and interested. Their trains of associations were far more vivid and their imaginative processes were more in evidence, remodelling, often completely recreating the perceptual material of the blots. And again, their emotional and valuing responses were more to the fore. The percentage of answers given on the basis of colour was higher. On the whole they preferred the coloured blots, whilst the Fd type preferred the uncoloured ones. It seems quite clear that this fundamental predilection for colour extends right through the various levels of the personality, from the physiological upwards. The Cd type also interprets the blots as a whole more frequently, giving no *Dd* answers. Often they weave fantastic stories to explain why they see the blot as a mermaid fighting with an octopus, and, as they draw in more and more of the picture, actually see changes taking place in the blots.

The comparison of our Cd and Fd types with respect to introversion and extroversion is also satisfactory. The more scattered results of the comparison in the case of the mixed groups (*CF* and *FC*) indicates that they are mixed types belonging to the various sub-forms of the integrate type. The tendency towards disintegration, or functioning through partial response systems, shows itself more and more strongly as we proceed from the *C* to the *F* type, and this again shows that an exclusive classification into two or three types such as Kretschmer's is not possible. Jaensch's types fit into a more biological classification, in that there are gradual transitions from one to the other. Form and colour dominance, as shown up in the tachistoscopic tests, are therefore stigmata, which are not necessarily present, or indicative of type, except in the case of pure colour dominance. It is most certainly not true, as Scholl states, that no schizothyme is Cd.

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[1] M. M. Said, "The specific nature of colour and form memories," Ph.D. dissertation, Birmingham, 1929. (I am indebted to Prof. Valentine for lending me his library copy.)

The exceptions are interesting from special points of view. They point the way to a closer examination of some weaknesses of character and will, and may help to clear up the problem as to how deeply repressions and repressed, *i.e.* dissociated, complexes affect the psychological structure of a personality. 'Autistic' types in the narrower sense of the word may be abnormal in this sense, or they may be normal types in whom the splitting off of feeling and emotion from the perceptual and imaginal levels has not yet taken place, as seems to be the case in the two synaesthetic types analysed by Jaensch and Weber[1]. Their study also leads in the direction of differentiating between 'formal' and 'material' types. The *C* and *CF* groups were predominantly students of art and literature, the *F* types of mathematics, physics and engineering.

(*b*) *General observations.*

In his paper on perceiving and imaging, p. 255, Bartlett remarks, that "separation into types, though it is of considerable practical value, solves no theoretical problem. The relatively set ways of reacting which are illustrated in types have practically always been acquired gradually. Whatever has a history presents a problem. And it is no solution to stop the history at some point or other and give a name to what has been found. The really interesting thing is to try to show how the different characteristics that are found together in a 'type' are connected, and in what way they have probably grown up." In the fifteen years since this was written the classification into types has proved its practical value over and over again. Moreover it has led nearer to a solution of fundamental problems than was dreamt of then, and has shown that many of the most fundamental characteristics of the minds of different types, at low and at high levels, have not been acquired gradually by the person, but are the expression of functional differences deeply rooted in the whole psycho-physical organism. Wherever similar differences in experiments at lower and at higher levels show themselves, this may be an indication of permanent *functional* characteristics.

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[1] *Über den Aufbau der Wahrnehmungswelt*, part II, Leipzig, 1931.

In the investigations recorded here, it was found that there is a gradual increase in the effectiveness of form perceptions between two extremes, the Cd and the Fd types. In the first type primary colour perceptions play a great part in all perceptual and imaginal activities, in the latter they play hardly any part. Through the various groups, *C*, *CF*, *FC*, *F*, we can trace this gradual increase, and the beginnings of the splitting up processes. It is as yet too early to see clearly what the exact roots of Cd are. But there are several indications of where we must seek.

It seems as though colour and form are originally indistinguishable elements of our perceptual apparatus. Their differentiation first takes place at higher levels. A study of the subjects in the *CF* group shows that they can be more form than colour dominant where it is a matter of interpreting complex sense data, whilst at lower levels, between the purely physiological and the perceptual, they are still colour dominant. Colour is a more immediate sense datum than form, in the perception of which many complex factors such as naming, meaning, attitudes, and attention play a part. It is therefore probably a more primitive function[1], or, if this seems too far-reaching a deduction, at any rate an earlier faculty in man than form perception. Colour is easily handled by the visual imagination, as the experience of child psychology, and especially the study of eidetic imagery shows. That the colour function is deeply rooted is also shown by the fact that the will has no influence on the capacity to select according to form or colour in the case of Cd subjects. There are no optical or physiological differences as far as we can see in the two types of subjects. Yet the *Sinnengedächtnis* (sensory memory), to use Helmholtz's term, selects one sense datum and not another, even under pressure from the will. At higher conscious levels we find that Cd's are more 'interested' in colour. But it must be remembered that the word 'interest'—from the observer's point of view—merely characterizes the objective fact that the organism does not receive every possible impression into itself. Even at physiological levels that are independent of will or interest, selection takes place; what is selected or rejected does not depend on optical or physiological factors, nor does it depend on the will. It depends, as it seems, on a far deeper teleological order. Some weaker Fd subjects can devote their attention exclusively to colour, but have some difficulty. This shows that the 'splitting up' process has not yet been completed, and also that the Cd's are integrate with respect to the optical sphere. Their integration is outward, that is, they are completely and uncritically open to sense impressions from without, which are at the same time not seen as invariants and therefore easily modifiable by associations, particularly emotionally toned ones. The Fd's are not integrate with regard to the optical sphere, and forms are therefore invariants for them.

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[1] Animals can more easily be trained in colour than in form discrimination, according to the experiments of Katz, Kroh and Scholl. The latter found that chickens conditioned to eat off a blue triangle but not off a red circle, immediately eat off the circle but not off the triangle when the colours are interchanged. Lashley has performed similar experiments on rats. General observations also tend to show that animals do not use their form discrimination as much as they might. They do use colour and motion, and these have an obvious biological significance in the struggle for existence.

If this is so, we should expect to find significant differences in the structure of the space perceptions of our two types. Those in whom space co-ordinates on the retina are fixed, *i.e.* in whom transverse retinal disparity plays a large part in determining depth, should be form perceptors. Those in whom space perception is still of the early, plastic form frequent among children, in whom the later 'petrefacts' have not yet been crystallized out and split off from the rest of their mental contents, should be primary colour perceptors.

(c) *Further experimental indications.*

The account of a relatively restricted experimental enquiry must stop at some point; and the investigation of the field of space perception would have to be a comprehensive one in itself. It was not possible to tackle that as well. But two preliminary investigations were undertaken with a limited number of subjects of the pure form and colour types.

They were first asked to look at a picture and at a solid cube through 'Rollett plates'. These are merely two plane parallel glass slabs of high refractive index, with dimensions 25 × 10 × 3 cm.[1] They were mounted on a turntable, so that they could be quickly changed from convergent to divergent positions. The effect—due to refraction through the glass—is that when the plates converge, as seen from the subject, the object will appear to be farther away, and nearer when the plates diverge. Only the degree of convergence or divergence of the eyes is changed; the size of the retinal image remains constant. Now when a subject looks through these plates first with near vision and then with more distant vision while the retinal image remains of the same size, one would expect depth to be more strongly seen, as well as an increase in the size of the picture. Nearly everyone saw a slight change. The Fd subjects on the whole saw very little, but the Cd subjects saw the most striking changes. "The picture has more life. I can almost see the people running about," one of them said. Subject 23, who easily saw movement in a picture, saw the most vivid movements taking place. The Rorschach blot in which she had seen most movement before, now became "a positive whirlpool of colours; I can't see any shapes, it is all moving around too fast." A cube was seen by her to become very squat with converging eyes, and enormously extended with diverging eyes, "like a long four-cornered rod." This shows what a great effect integratedness in the optical sphere can have. When the eyes diverge, we naturally tend to feel that an object is farther away. Any tendency to over-estimate this is counterbalanced by the transverse disparity factor, which remains constant, or nearly so. But in the case of integrates, which our Cd's obviously are, the idea of distance is far stronger than the physical stimulus and they see what they think and feel. "See movement? How do you mean movement?" we were asked by subject H., an engineer. We explained; "Well, anyone who can see these things moving about must be soft in the head," was his summing up.

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[1] Cast by Parsons Optical Glass Co., Derby.

We also examined the after-images of these subjects, and found that the time for which they persisted was, on an average, three times as long in the Cd's as in the Fd's. This, too, is characteristic of integrates, as the Marburg researches have shown.

A further observation was recently made, at first accidentally. Some psychology students were shown the phenomenon of 'simultaneous contrast.' A blue and a yellow square lie side by side. On them is placed a strip of neutral grey paper, and the whole is then covered up with very thin tissue paper. The strip of grey now appears to be yellow over the blue square and blue over the yellow square. If the tissue paper is not used, it is hard to see the simultaneous contrast. The reason seems to be that the grey strip looks too definite; there are shadows around it and it stands out in three dimensions. To see the contrast without tissue paper it is necessary to press the grey strip so that it lies flat on the coloured squares, and hold the whole thing so that no shadows can be thrown.

Close on this followed a second observation. When the squares are taken into the shadow—the room not being very light to start with—the Purkinje phenomenon manifests itself quite clearly. The blue field seems much brighter than the yellow. But as soon as a form is introduced into the blue field, such as the grey strip, or any small object, there is an immediate decrease in the apparent brightness of the blue, so that it approximates again to the apparent brightness of the yellow. Both these effects—the diminution of simultaneous contrast, and the reduction of the Purkinje effect—seemed to be less pronounced in the case of our Cd subjects who were present. As far as could be judged, they also had more pronounced simultaneous contrast than most other people.

This suggests several very important conclusions, which will have to be experimentally tested.

(i) The Purkinje phenomenon manifests itself during total or partial dark adaptation. That is, in the light-adapted eye, the energy maximum of the spectrum lies in the yellow. In the dark-adapted eye this maximum shifts towards the blue. Now scattered light contains a higher percentage of short wave-lengths than direct daylight. So the function of the Purkinje effect is obviously to utilize these to the greatest possible extent.

(ii) But forms are much less easily distinguishable in blue light than in yellow or red. When a form is put on to a field that looks brighter to a dark-adapted eye than a field of equal intensity but longer wave-length, this difference is greatly reduced. Thus, as soon as the form function of the eye is active, the eye functions as it does in daylight vision, and the energy maximum again shifts towards the red end of the spectrum.

(iii) The same thing happens in simultaneous contrast. Therefore the functional roots of both the Purkinje effect and simultaneous contrast may be the same.

(iv) The fact that a Cd does not notice these effects to the same extent, means that the form function of his visual apparatus is not yet as firmly developed as it is in the Fd. If this is borne out by exact experiment, it will be a proof at a very much lower level than was reached in the tachistoscopic experiment.

One last observation may be made. A study of the gradual recession of colour dominance may lead to interesting conclusions in the psychology of painting. Strong Fd's abstract colour to such an extent that they cannot perceive the colour of shadows. This is borne out by some of the interpretations of Rorschach blots. If this is followed up we may get some insight into the psychological basis for the differences between impressionistic, expressionistic, and realist forms of painting.

(*d*) *Ontotropic and philotropic selective principles; attention, attitude, perception.*

Experimental work on the higher mental processes is difficult and uncertain, because of their enormous complexity. The mind is capable of dealing with a situation in so many different ways, that one can never be certain that a given result is an answer to the question proposed by the experiment. The same answer may be due to totally different causes at different times and in different people. The most striking phenomena among the higher mental processes are teleological tendencies, and in these we must include the constant tendency towards meaning, and the apprehension of values. These factors overlay and obscure factors springing from the lower physiological and physical levels to such an extent that extrapolation, or inference from the higher to the lower, becomes very misleading or even impossible. On the other hand, experiments that are made on very primitive physiological levels, also do not allow of wider generalizations about the higher levels. The finer differences of personality, for instance, begin above the level of sensory thresholds. But there are levels of experience, which at one end are still deeply rooted in experimentally investigable psycho-physiological events, and at the other extend into the higher levels. It is at these levels that experiment promises most insight.

This thesis can best be illustrated by the special case of Eidetic Images (*EI*'s). They are phenomena that lie midway between true physiological After Images (*AI*'s) and pure Memory Images (*MI*'s), and have the properties of both to varying degrees in different personalities. Their close study as the extensive researches carried out at Marburg, Vienna, Hamburg and elsewhere have shown, give a considerable degree of insight into the formal structure of these intermediate levels. It is found that in them certain principles can be traced that are of importance for the description and classification of human types. Their study has also shown that certain selective principles operate at levels far lower than was generally believed to be the case. In all our higher perceptions and mental actions generally a very high degree of selection is continually taking place. What is selected by the mind from the innumerable stimuli constantly impinging on it depends to a great extent not on the stimuli as such, but on purely subjective factors like meaning, values (*e.g.* conscious and 'unconscious' wishes and interests), emotional states, attitudes, and previous experience. To a large extent, however, the will can determine what is selected even against all these influences. In *EI*'s we find that selection also takes place. Here again, what is selected does not depend on the strength of physical stimuli at all, but on teleological and valuing tendencies. Moreover, selection is quite independent of efforts of will, which shows that even lower sensory levels are not as free from teleological tendencies as mechanistic materialism asserts.

This may be made clearer by considering what Jaensch has called 'ontotropic' and 'philotropic' selective tendencies in perceptual processes[1]. 'Ontotropic' selection is selection at lower levels directed towards the apprehension of objects. Helmholtz already indicated that in space perception the primary fact is not the registration and summation of disparate stimuli, but the perception of objects (or 'wholes'), and v. Frey showed that similar laws hold for tactile perception, while Jaensch proved it in colour vision. The phenomena of colour constancy and colour transformation are due to the immediate selection by the eye or the visual processes of meaningful wholes. Now in the special sphere of *EI*'s this ontotropic selective tendency manifests itself as follows: Eidetic subjects cannot produce images of meaningless things, such as a number of pieces of coloured paper stuck together on a card, and some can only get *EI*'s of solid objects. If they get *EI*'s of homogeneous coloured surfaces, these either fade very quickly or appear in complementary colours, like *AI*'s. Thus, for example, they can produce a negative *EI* of a green piece of paper cut in the shape of a leaf, or at most a dim and fairly short-lived positive *EI* that soon becomes negative or vanishes. But when a real green leaf of the same colour and saturation is substituted they have no difficulty at all. The subjects themselves are surprised at this, but no effort has any effect. Similarly, if a picture is presented to them, in which some part has been substituted by an incongruous geometrical figure, this part drops out of the *EI*, or appears in its complementary colour. Only in the most extreme cases at both ends does this not happen; with subjects who have extraordinarily strong *EI*'s, or only complementary (negative) *EI*'s, selection does not seem to take place.

'Philotropic' selection is of a somewhat different order. It is, to use Jaensch's term, *persönlichkeitsadäquat*, in accord with the personality. In *EI*'s it manifests itself in the ability to produce *EI*'s only of certain objects, and is accompanied by strong feeling. Thus some subjects can reproduce images only of flowers, or of certain kinds of houses of which they are particularly fond. In other cases it is found that unpleasant faces or colours in pictures drop out in the *EI*, to be replaced by others more in harmony with the predominant likes and dislikes of the subject. In ontotropic selection, then, we have a direction towards the objective facts, in philotropic towards subjective values. Now colour and form are also selective principles. They are specific unitary functions independent of each other, and of factors like Spearman's '*G*.' Where they manifest themselves in their pure form, as in tachistoscopic experiments, they appear separately in different persons, and these persons show very striking psychological differences. The Fd subject is guided by the ontotropic selection principle not only as regards the particular form function, but throughout the various levels of the 'personal unity,' while the Cd is under the influence of the other, subjective, principle[1].

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[1] E. Jaensch, "Über psychische Selection, etc." *Zeitsch. f. Psychol.* 1925, p. 98.

Considerations such as these suggest that the important factor to observe in psychic life is not attention, but attitude. There are two main forms of attention: simple 'taking notice,' and a critical, concentrating intellectual attitude. The latter is usually meant by the term 'attention.' Now in reading experiments[2] in the perception of form or colour in short exposures, and in the interpretation of the Rorschach blots, all our subjects 'attended' to the presented material. One type of subject saw what was objectively present, another type saw something entirely different. Results like these have usually been attributed to two kinds of attention: 'synthetic' or 'dynamic,' and 'analytic' or 'static.' But a study of the mode of response in the two extreme classes of subjects and of the records made by them shows quite clearly that what we are dealing with is not two kinds of attention, but completely different ways of perceiving. The 'subjective' person does not direct his attention first at the screen or the blot, to perceive a few letters or vague shapes, and then at his aroused association, until he finally 'synthesizes' those that best fit together. He immediately *sees* a meaningful word or picture. Attention has an essentially disintegrative function. If I 'attend' to an emotion, it gets weaker or disappears. If I attend to a symphony I can at will lift the separate instrumental tones out of the flowing mass of music by a critical, analytical attitude; but the primary 'wholeness' of the musical impression is thereby destroyed. It seems that all experiments designed to measure the function of attention by measuring its field are misplaced. What we

[1] The term 'personal unity,' like Leibniz's '*unitas multiplex*' describes more than I, or ego, or personality. It includes all the various functions, conscious and 'unconscious,' that in their sum constitute the psycho-physical organism.

[2] Cf. footnote, p. 299.

measure are perceptions and intermediate integration products, that is, the 'field' of attention. The concepts 'wide' and 'narrow' describe something purely external, the contents of attentional acts, but never the function of attention. It is easy to show that in reading experiments subjects of the 'analytical' type have as wide a field as subjects of the 'synthetic' type. They can both see the beginning and end of very long words equally well. Similarly, in the form-colour experiments both Cd and Fd subjects glance over the whole field. The fact that, from the observer's point of view, one type records far more letters and figures says nothing at all about the 'width' of attention. In the first place, as we have seen, the two types perceive in different ways. Secondly, the factor of 'subjective certainty' is of very great importance. In this factor ontotropic and philotropic tendencies again show themselves very clearly. The basis for subjective certainty in the Fd analytic, objective types is the satisfactory correspondence and comparison of a perception with objective reality; for the Cd, synthetic, subjective types it is above all the feeling-quality of the integration products that so frequently form part of their perceptions. Bartlett, in the discussion of his experimental results remarks (p. 244), "there is a strong tendency to refer change in the sensible appearance of objects to one's own faulty observation." This is true of all the more disintegrate types, but is not true of children and integrates. They have no difficulty at all in believing that objects can change their sensible appearance, since they are (apparently) continually doing so. Lastly, the Fd is much more cautious in any statements he makes, and tends to suppress observations of which he is uncertain. One never seems to find evidence of attention being any other function than that possessed by all organisms whatsoever, 'simple noticing,' or 'simple awareness.' This must operate before any of the higher functions can have any material on which to operate. We cannot will or analyse or criticize anything unless we have already been made aware of it by an act of attention. That is its function; and what follows has nothing at all to do with attention. It is the effect of various attitudes[1].

We have said that observations of general behaviour, moods, expressions, and gestures, should form an integral part of any experiment. We can only *know* their import objectively through the direct statements of subjects. This is certainly a return to introspective methods, but with this difference: the introspections of subjects are significant as expressions of the mode of experience and thinking of their particular type, significant, that is, to the experimenter who is closely observing the subject taking part in an experiment. The introspections have no *general* validity except for a particular type: Anyone who for any length of time has pursued experimental enquiries typologically knows how closely each one of us is shut up in the circle of his experience. He is 'caged in his type,' as it were. If the introspections of a subject belonging to some pronounced type are shown to one of the opposite temperamental type, it will often be found that there is no point of contact. The two subjects cannot understand each other's experiences, just as a dull, worn out miner may be incapable of understanding the finer emotions of a poet contemplating a sunset.

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[1] This thesis is developed in greater detail in O. Oeser, "Tachistoskopische Leseversuche, als Beitrag zur struktur-psychologischen Typenlehre," *Zeitsch. f. Psychol.* 1929, p. 112.

In his presidential address to the Psychology Section of the British Association at its South African meeting in 1929, Prof. Bartlett mentions what must be a common experience of all psychologists: that subjects differ, but that in a group of experiments one will find one subject always accurate, while another varies. He thinks that this ought to be examined by psychologists, because the attitude that the subject brings to an experiment is very important, and comes to the conclusion that the important thing for experimenters to realize is the constancy of attitude, not of experimental conditions. Without doubt, attitude *is* the most important factor of all. We may call 'the total organism' a 'foggy' conception and imagine that we are always dealing with 'partial response systems' such as sight. But even in simple experiments on colour transformation, the abstraction of light and shade ('constancy of brightness'), and the like, central factors become of such importance that we have to make the conception of a partial response system very wide indeed. Much more so when we are dealing with such integrations as synaesthesia. Head, Rivers and Adrian have demonstrated the very wide ramifications of any high-level function. Jaensch has shown how temperamental tendencies, and, above all, attitude, dominate every response up to the highest. From this point of view, the conception of the total organism being active in any response assumes a very different character and a deep significance. For we find that the attitude of a subject toward an experiment or a series of experiments is extraordinarily constant, and is traceable in all responses, inside and out of the psychological laboratory. But we also find that the idea of the 'total organism' being active is not applicable to every subject. It is true of the 'integrate' type alone. Only in the so-called 'objective,' or more disintegrate type of subject can we truly speak of partial response systems, because his functions are, as it were, in water-tight compartments. But an observer of the objective type cannot know this *a priori*, since his is the partial response reaction and is the only one he knows in direct experience. Through careful observation and analysis of the behaviour and the introspections of another type, however, he will come to realize this fact, and make allowances accordingly[1].

(e) *Suggestions for method in experimental procedure.*

If series of psychological experiments are carried out from a typological point of view, it brings about a large number of simplifications, unifies widely different fields of experimental enquiry and reconciles conflicting results. It throws much light on such problems as adjustment and readjustment in continuous reaction experiments, on selective principles, and on structural levels, while the question of attitude and instruction takes on a new importance, and the problem as to the nature and function of attention is being placed in a different light. What were thought to be different modes of functioning of attention, are found to be expressions of functional structures in different psychological types. Rigorous experimental typology is also leading to a deeper understanding of many of the great antitheses between such widely different points of view as are represented by idealistic and materialistic philosophies, because of the profounder insight into the teleological structures of personality afforded by the experimental study of human types.

The following suggestions for the procedure in an experimental investigation may now be formulated.

(1) Simple tests and 'clinical' phenomenological observations of the subjects, to determine roughly to what type they belong, should form the starting-point.

(2) The constancy of attitude being more important than the constancy of experimental conditions, it follows that care must be taken to give instructions in such a way that they can be conformed to normally, without psychological strain, by the subjects. That is, the instructions must conform to type. This may mean that some subjects may have to do only part of an experiment at one sitting, as was the case with some Cd's in the Rorschach tests.

(3) The attitude set up by the instructions must be very carefully noted.

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[1] Cf. O. Oeser, "Typological Methods in Experimental Psychology," *Psyche*, (1930), XXXIX.

(4) The interpretation of results must also be subordinated to the typological point of view. Generalizations are usually valid for one type, and for one class of responses only, provided the instructions have conformed to type.

(5) Experiments in which instructions do not conform to type (as in Grossart's work on reading), have to be very carefully interpreted, since they may give an altogether false picture. They may be used for determining the flexibility of a particular type, or the degree to which the mental functions and contents can be 'split off' from one another.

(6) All phases of the development of the personality must be subjected to accurate phenomenological analysis in each type, and typology must be differentiated with respect to the structure of the various faculties and psycho-physiological dispositions.

(7) Trends and tendencies are to be indicated, rather than dogmatic laws.

(8) The genetic approach should never be lost sight of. That is, similar experiments should, if possible, be carried out both on adults and on children.

(9) The descriptive methods should, as far as possible, be combined with the experimental, for instance by means of questionnaires and records of conversations.

(10) Wherever possible, more than one experiment should be done with the same groups of subjects.

(11) Systematic theory should come last.

V. SUMMARY.

In tachistoscopic experiments[1] in which the form and the colour of a figure to be selected from a collection of figures have been split apart, some observers always find the colour pole (Cp), others the form pole (Fp). The former have been called colour dominant, the latter form dominant. There are two mixed groups: the one is usually attracted to the Fp, but perceives its colour as well; the other is usually attracted to the Cp, but frequently perceives its shape. When intense forms and weak colours are used, the latter group more frequently find the Fp. When intense colours and weak forms are shown, some of the more Fd subjects become Cd. But wide differences in the material do not affect pure representatives of the Fd or Cd types. Men were found to be more Fd than women. Scholl's statement that all schizothymes are Fd does not seem to be true of Cambridge undergraduates.

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[1] "Some experiments on the abstraction of form and colour," *Brit. J. Psychol.* 1932, XXII, 200.

These results are confirmed by using Rorschach's blot tests. The Fd and Cd subjects are found to be very different personalities in many ways. The Cd's, again, give far more interpretations on the basis of colour; what they see often bears little resemblance to the actual blots. Feeling plays a great part in their interpretations. They are better able to interpret the blots as wholes, give more motion (kinaesthetic) answers, and are all 'extroversives' on Rorschach's classification. The Fd's interpret on the basis of form, select small, clear details, and are more 'introversive.' Form and colour as selective principles in perception can therefore function independently in different individuals, both at lower and at higher levels. Colour is seen to be a philotropic, form an ontotropic selection principle. The Cd's are integrates, as subsidiary tests of their after images and space perceptions show. But the Fd groups contain several mixed types.

The difference between the numerical distribution of the Fd and Cd types here and in Wurttemberg suggest that these tests could be used as group tests on a large scale. The possibility thus arises of carrying experiment into the field of social psychology.

The suggestion is made that further experiment may show the Purkinje phenomenon and simultaneous contrast to have the same functional roots in an early integrated state of the visual perceptions.

Constancy of attitude is shown to be the most important factor in psychological experimentation. The 'width' or 'narrowness' of attention has nothing to do with attention as such, but is the result partly of attitude as expressed in the different criteria for 'subjective certainty' in the two main types, and partly of their different mode of perceiving. Such terms as 'wide' and 'narrow' describe the purely external registration of results by the observer, and not the actual experience of the subjects.

(*Manuscript received* 21 *October*, 1931.)

THE INFLUENCE OF VOLITION UPON THINKING[1].

By F. AVELING.

(From the Psychological Laboratory of University of London, King's College.)

I. *Some previous researches* (p. 324).
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I. Some previous researches.

The work communicated in the present paper is the outgrowth of a number of previous researches carried out in the laboratory of King's College on the influence of conation and affection upon mental and bodily processes, and in particular of those of Messrs Saidullah, Stevanović, Messer and Caws, which have already been published or reported elsewhere. In accordance with the conclusions of Ach, whose researches upon the will established the existence of 'determining tendencies' which follow upon volitional acts and, though sinking beneath the conscious threshold, tend to secure the carrying out of the willed activity, we found that other processes than those investigated by him were influenced in the same way. Thus, for example, Saidullah showed that it was possible by willing to increase the span of apprehension; Messer proved that the time of presence of one of several alternating visual after-sensations could in a similar manner be lengthened at the expense of the others; and Caws showed that willing to perceive a given symbol, exposed tachistoscopically among a group of symbols too great to allow of simultaneous perception, increased the likelihood of its being perceived. It was especially the last-mentioned research which suggested two of the three investigations here to be reported.

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[1] Communicated to Section J (Psychology) of the British Association for the Advancement of Science, 1931.

II. CONTROLLED THINKING AND REVERY.

Though it is a commonplace to say that our thought is largely directed by will, in the sense that logically controlled thinking is absolutely different from revery, and intentional recall absolutely different from mere remembering (an observation as old, at least, as the time of Aristotle), remarkably little experimental work has been carried out in this direction. Our plan was, accordingly, to investigate the influence of willing upon the three fundamental processes of original knowing, viz. the cognizing of experience, the awareness of relations between the items of experience (whether cognized or merely lived), and the production of ideal correlates. It will be convenient here to deal in the first place with the last of these.

III. INFLUENCE OF WILLING.

(a) *Influence upon eduction of correlates.*

It is well known that willing (intending or resolving) tends to influence the emergence in consciousness of ideas in definite relation with any given idea. This is abundantly demonstrated by all the so-called "Controlled Association" experiments. The term 'association' is here a misnomer; and the experiments in question should more properly be called those of Relationally Predetermined Reaction. It is also well known that, having voluntarily accepted an instruction to react, say, with their opposites, to words given as stimuli, after the first few reactions the opposites tend to come to mind without further conscious reference to the instruction. This in itself would be enough to show the influence of willing upon ideal correlate eduction, whether noegenetic or merely noetic. But we were anxious further to investigate three points: (1) Does the relation determined by the instruction function subconsciously? (2) Do the different relations function in the same manner? (3) How does the process take place?

We experimented with Free and Relationally Predetermined Reactions, in the latter of which we used the relations of opposition, similarity, attribution and causality. There were eight subjects and, in all, 680 reactions; not a large number, but sufficient to yield introspections adequate to our purpose. Since the method of experiment is familiar it need not be described here, and the principal relevant results may at once be recorded.

(1) The relation guiding the reaction sinks quickly beneath the threshold of consciousness, yet still functions, in all these types of

reaction. It tends, however, to reappear consciously in certain circumstances; as, for instance, (*a*) when the subject is badly adapted; (*b*) when there is a hitch in the process due to such factors as inordinate temporal length, occurrence of irrelevant thoughts, and the like; (*c*) when the reaction *idea* is incorrect (*i.e.* is not in the required relation to the stimulus idea).

(2) In these respects the different relations appear to function in the same way, as well as in respect of the fact that a *notional* awareness of the meaning of the correlated reaction word tends to arise in consciousness *before* the word expressing that meaning. Subjects frequently report having had to search for the required reaction word to express the exact meaning.

(3) In all cases, but especially in that of relations of similarity, abstract concepts tend to arise under which stimulus and reaction meanings (and words) are seen to be subsumable.

The foregoing are introspectively reported by all the subjects.

The temporal values of the several kinds of reaction (averages) are as follows:

	Free	Opposition	Attribution	Similarity	Causal
Sigma	804	1330	1284	2182	2862
Order	1	3	2	4	5

Though the order of attributes and opposites, and of similars and causals, varies from one subject to another, attributes and opposites present, seemingly, less difficulty than similars and causals, as might, perhaps, have been theoretically expected; but, curiously, in contradiction with nearly all previous findings, all the Relationally Predetermined Reactions are longer than the Free ones; and this is so not only in respect of the pooled average reaction times of all the subjects, but in the case of each individual subject also. The conclusion to be drawn here is that a previous volition affects the process of correlate eduction by means of a conative attitude, or 'set,' which may be wholly subconscious. Since they were initiated by volition, such conative attitudes should properly be described as volitional tendencies or dispositions; and they may be compared with connate instinctive dispositions as these latter are usually conceived, in that (1) there is consciously present a significant stimulus, or situation, into which there is *insight*; (2) there arises, more or less clearly, an end, or goal idea correlated with this, in respect of which there is *foresight*; and (3) such goal idea may be evoked by a relation which functions beneath the level of consciousness.

(b) *Influence upon cognition of experience and eduction of relations.*

We turn next to the two investigations of the influence of willing upon the cognition of experience and upon the eduction of relations. In these investigations we had altogether 18 subjects, all students or teachers of psychology; but use was made of the data obtained from 16 only, as one was not able to complete all the experiments, and another finished them too late for the inclusion of his data in the calculations. Heuristic use, however, was made of his very valuable introspective reports.

IV. EXPERIMENTAL PROCEDURE.

Since similar methods were followed in each of these investigations, a single description may be given for both, though the results must be considered separately.

Groups of monochrome letters and of monochrome and coloured symbols were drawn on slides, and exposed by means of a Michotte rotary projection tachistoscope for an interval of 50 sigma. Each group consisted of nine items (letters or symbols) arranged in three rows and columns of three items each, thus forming a square; the whole of which was in detail well beyond the span of apprehension of the subjects. There were three series of such groups, consisting of 24 slides each.

Two instructions, the effects of which were to be compared, were given to the subjects. One was as follows: "Observe all you can, and record what you see." The second was "Look for a given letter (or symbol or relation), if it should be shown, and record all that you see." The technical procedure was invariable. After the instruction had been read out, a signal, 'ready,' was given, and at 'now' the exposition was made. Immediately afterwards the light was switched on—the actual exposition having been made in a darkened room—and the subject recorded his observation by drawing what he had seen, making introspective notes also of any features of the processes of perceiving, holding, and recording the items which seemed to him to be relevant.

In the case of the coloured symbols (which were used for the investigation of the influence of will on the eduction of relations) there was a known number of actual relations of similarity between the items in respect of colour and in respect of shape. Similarity was the only relation with regard to which the experiments were made.

When all the expositions were completed, the results were scored as follows. For every item (or relation) correctly indicated two marks were given, one mark being subtracted for every error in location and also for every item reported which had not been exhibited. The total possible scores for each subject were:

(1) For monochrome letters and symbols, 216.

(2) For coloured symbols, relations of similarity in respect of colour, 96.

(3) For coloured symbols, relations of similarity in respect of shape, 136.

(1) Individual scores for the "Observe all you can..." experiments were divided by the total possible scores, thus yielding percentage scores for each subject. These percentages were taken to indicate the expectation that any one given letter, or symbol, or relation, would be seen. I have termed this the *Expectation Index.*

(2) Scores for the "Look for..." experiments were made in the same way. These scores subtracted from the Expectation Index give a *Shrinkage Index,* showing the *loss* in the total amount seen when the subject is set to observe a given item.

(3) Finally, in the "Look for..." experiments results were scored for the letter, symbol or relation relevant to the instruction; and the percentage of successes $x/24$ was calculated. This may be called the *Success Index*: and it indicates, when the Expectation Index is subtracted from it, the *gain,* over and above expectation, for the letter, symbol or relation for which the subject was looking.

V. RESULTS.

The results show a very considerable gain for the items which the subjects had determined to apprehend if they were exhibited. The average gain pooled for all subjects is as follows:

Letters	Symbols	Relations (colour)	Relations (shape)
22 % (P.E. 3·1)	47 % (P.E. 2·7)	23 % (P.E. 2·9)	33 % (P.E. 3·9)

On the other hand, the losses were:

8 %	3 %	7 %	0 %

The fact that in four instances only out of the total number of pooled experiments was a gain not apparent warrants the conviction that the resolution to apprehend an item, or relation, if it were given, really has an effect upon the subsequent perception. The inference is that the will-act sets up a conative tendency in virtue of which an object, or relation, if it should happen to be in the presented field, is more readily apprehended. It tends, so to say, "to leap to the eye." This phenomenon cannot be explained by any theory of preperception, imaginal or otherwise; for this could not account for the fact that it occurs no matter what the position of the item, or of the fundaments of the relation, within the visual field. Wherever the item may be, or between howsoever located fundaments the relation may obtain, there is a tendency for the item or relation required to be apprehended, when a previous resolution has been made to apprehend it if it should be there.

On the other hand, in these conditions of the experiment, there is a general loss, though as a rule slight, in the total amount of material apprehended. With regard to this loss, it may be asked whether it is a definite loss in amount of apprehension, or a loss in the amount of material retained during the period elapsing between the actual apprehension and the recording of what was seen. Subjects report that when they were looking for a letter or relation they actually *saw* far more than they were able to retain long enough to record. And it may be that this introspective report is an accurate account of what in fact happened. Such loss could be explained on the ground either of a drainage of mental energy towards the item for which the subject was looking, with or without drainage away in the conation itself, or of a drainage of energy in holding the required item, which would result in a speedy sinking beneath the threshold of those items or relations which were not relevant to the main task. Either or both things may have occurred. For the inference I wish to draw, however, it is practically immaterial which explanation is to be accepted.

The gains over the calculated expectation of any given item, letter, symbol or relation range in individual cases from 69 per cent. to nil, and even, in 2 cases out of 1536, to very slight minus quantities. The inference here is not only that there are very considerable individual differences, but also that the influence of willing varies according to the difficulty of the performance. Thus the gain in the case of apprehending letters and relations of similarity in respect of colours (22 and 23 per cent.) is less than that in the case of apprehending symbols and relations of similarity in respect of shapes (47 and 33 per cent.). But, on the whole, the symbols were less familiar than the letters, and the shapes less familiar than the colours. Moreover, there were fewer possible relations in the case of the colours (96) than in that of the shapes (136). The Expectation Index shows that the order of difficulty is as follows:

Letters	Relations (colour)	Symbols	Shapes
38 %	27 %	21 %	5 %

And the gains are, with what is, I believe, an explicable exception, exactly in the reverse order, viz.

Letters	Colours	Symbols	Shapes
22 %	23 %	47 %	33 %

It is suggested that the reason why the gain in symbols is greater than that in relations (shapes) is that the latter was in point of fact a task almost too difficult to perform. The average Expectation Index here was only 5 per cent.

In any case, it would appear to be quite certain that the antecedent will-act, as in the case of correlate eduction, influences very considerably the process of perception and/or the retention of items and relations if these should have occurred in the presented visual field.

There is a further observation which appears to be of the greatest interest and importance. Relations were correctly reported again and again as obtaining between fundaments, one or even both of which had not been clearly apprehended. Some subjects reported this fact categorically; others said that they *had* apprehended the fundaments, but had forgotten them (though they remembered the relation and its spatial position) before they could draw them. Certain it is, however, that whatever happened, or if both happened at different times and with different subjects, a relation *can* emerge clearly in consciousness and remain clear in the absence of one or both of its fundaments. Again, however, for the inference I wish to draw it is practically immaterial whether the relation is clearly apprehended in the absence of clear apprehension of one, or both, fundaments, or whether it is apprehended together with its fundaments, which incontinently sink beneath the conscious threshold, leaving it attached to one fundament, or even alone, in clear awareness. A very interesting point in this connection is the fact that the relation apprehended might lie between fundaments situated in any parts of the visual field. Subjects often reported that they were aware of eye movements from one fundament in the direction of another, although no eye movements were possible during the time of the exposition; and this especially was the case when the actually related fundaments were situated far apart in the field, and when the consciousness of one of them was asserted to have been absent. With regard to the averages which have been presented in this communication, the significance of the differences between the means was calculated by the usual formula: $\sqrt{PE_x^2 + PE_y^2}$, with the following results:

	Letters	Symbols	Colours	Shapes
'Gain'	22 % (P.E. 3·1)	47 % (P.E. 2·7)	23 % (P.E. 2·9)	33 % (P.E. 3·9)
'Loss'	8 % (P.E. 2·1)	3 % (P.E. 0·98)	7 % (P.E. 2·1)	0 % (P.E. 1·1)
'Look' and Success Index	30 % (P.E. 3·6)	50 % (P.E. 2·7)	30 % (P.E. 2·8)	33 % (P.E. 3·8)

With the exception of the 'Loss' in the case of relations of similarity in respect of shape, all these figures are significant. There can accordingly be no doubt that the 'gains' are due to the antecedent will-acts; and, though it is not so strikingly manifest, that the 'losses' are so as well.

VI. THEORETICAL CONCLUSIONS: INSTINCT AND WILL.

In the section dealing with the influence of volition upon correlate eduction, we have already seen that an antecedent will-act sets up a volitional tendency to respond to a given stimulus, when apprehended with *insight* (*i.e.* as meaningful), by evoking an ideal correlate in definite relation to it. Such ideal correlate, we suggested, may be equated with the *foresight* postulated to explain the cognitive guidance of instinctive behaviour.

But in those Relationally Predetermined Reactions *single* meaningful stimuli (words) were presented to the subjects, just as a bone or biscuit might be given to a hungry dog; and they reacted to those relatively isolated stimuli by evoking the correlates, just as the dog would react by eating the biscuit or gnawing at the bone. There need be no consciousness of relation in the former case, as we have seen. And there is no necessity of supposing any in the latter. Neither the animal nor we, however, exist in the presence of isolated stimuli, but in a complex environment from which a selection of relatively isolated stimuli must be made in order, by a manipulation of them, to satisfy our natural wants and consciously entertained desires. How does this selection come about? Our experiments on the influence of volition upon the cognition of experience and upon the eduction of relations, I think, provide an answer.

If we will to observe a given object should it be presented among a number of other objects, there is a tendency for the consciousness of that object to be enhanced and/or held in mind at the expense of the others. If we are looking for a given relation, there is a greater probability of our finding it or, having found it, of holding it clearly in view. In neither case need the conative attitude of 'observing' or 'looking for' be a conscious one; though, of course, it may be so, and frequently is so.

Here we have enhanced selective *insight*, without which relevant *foresight* is not possible. Volitionally initiated dispositions of this kind, again it is suggested, are comparable with the connate instinctive dispositions as usually conceived, at any rate in so far as their cognitive aspect is concerned.

Volitionally or connately we are 'set' to "pay attention to objects of certain classes or kinds" and to certain relations obtaining between them; which objects, together with the relations, lead to the creation (or remembering) of their correlates. I would merely refer in passing here to the reactions of animals to *relative* brightnesses.

Indeed, it is from our own volitional experience that the concept of instinct is derived; for we have no direct knowledge of the working of the animal mind—its impulses, cognitions and feelings—but solely of our own.

The fact that we can analyse such experience in ourselves into conscious determination or desire, increased selective perception of objects which (by manipulation) may satisfy that determination or desire, and a relation which may lead to an ideal correlate to be realized by appropriate manipulation, forces us to interpret animal behaviour as due to closely or remotely analogous experience. And the further facts that, in our own case, items may function subconsciously in generating relations, and relations subconsciously in generating correlates, and possibly correlates also subconsciously in directing action, permits us to apply the analogy in the interpretation of animal behaviour, without the necessity of implying that the experiences of animals need be consciously cognized as sometimes our own are.

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THE MUSICAL TALENT OF NATIVES OF EAST AFRICA.

By RICHARD A. C. OLIVER.

I. *Introduction* (pp. 333–334).
II. *The tests* (p. 334).
III. *The subjects* (pp. 334–335).
IV. *Method of administering the tests* (pp. 335–336).
V. *Results* (pp. 336–343).
VI. *Summary* (p. 343).

I. INTRODUCTION.

THIS paper deals with an attempt to measure some of the fundamental capacities comprising musical talent, in natives of East Africa, and to compare these natives with people of European descent in respect of these capacities. There are difficulties in the way of any attempt to apply the same psychological tests to peoples at very different stages of culture. Usually a test includes elements which are not equally within the experience of two such groups; and it is then very difficult to abolish the disparity in experience by supplying information to the more ignorant group, without spoiling the test. A test of range of information, about the customs of one people and in its own language, would be an example of such a test *par excellence*. Some psychological tests, however, measure much narrower and simpler functions, and inequality is in them lessened or even altogether avoided. Measures of visual or tactual acuity, for example, need involve no elements with which civilized peoples are more familiar than primitive peoples. The measures of musical talent used in this study approach this ideal. They are measures of functions closely dependent on comparatively simple physiological mechanisms. It is true that they constitute problems whose understanding demands a certain degree of intellectual maturity, a certain amount of information. But the level of intelligence required is low, the fund of information small. The measures are not intended to constitute tests of either intelligence or information. If the examiner performs his functions properly, he will suit the instructions to the intelligence of his subjects, and impart whatever information may be necessary. It should be possible for him to ensure that a group of primitive subjects embarks on the actual test of ear with little or no disadvantage in comparison with a group of civilized subjects. This seemed possible at any rate with natives of East Africa, and in so far as it has been achieved the comparisons between them and people of European descent may be considered valid.

II. The tests.

The tests used in this study were the Seashore Measures of Musical Talent, comprised by six special gramophone discs. They are measures of the sense of pitch, the sense of intensity, the sense of time, the sense of consonance, memory for tones and the sense of rhythm—these being six capacities which Seashore's analysis isolates as fundamental to musical talent.

III. The subjects.

The subjects of the experiment were the pupils of the Alliance High School, at Kikuyu in Kenya Colony. The Alliance High School is a central school for boys, offering the most advanced education open to natives in Kenya and drawing students from all parts of the Colony. The number of subjects tested was ninety.

The exact ages of the students were known in only eleven cases, as it is only within the last few years that missions and other agencies have begun to prevail upon Africans in Kenya to register births. The principal[1] of the school, however, has estimated the ages of the other pupils from anthropometric measurements, and these estimates have been used. The ages, true or estimated, ranged from 12 to 24, with a mean of 19·75, and a standard deviation of 2·61.

The number of students belonging to each tribe is shown in Table I.

Table I. *Distribution by tribes, of ninety East African natives tested with the Seashore Measures of Musical Talent.*

Tribe	Number of students tested
Kikuyu	67
Akamba	10
Luo	7
Bantu Kavirondo	2
Munyasa	2
Mrabai	1
Masai	1
Total	90

Of the students' experience of European music, it is known that all are taught singing in school, and ten are taught to read music in staff notation and to play the harmonium. Further, forty-two students in Forms 2, 3, 4 and 5 had undergone the same tests three months previously, when the writer was trying to devise suitable methods of administering the tests to Africans.

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[1] Mr G. A. Grieve, to whom the writer's thanks are due for his permission to conduct the test.

IV. METHOD OF ADMINISTERING THE TESTS.

The tests were given to classes of from twenty to forty students at a time, each test occupying a school period of about forty-five minutes. The instructions were given in English, the language used in the school. The general procedure followed was similar for all six tests. First, the experimenter introduced the test by means of demonstrations on the harmonium and the blackboard, and in other ways, until the class found itself actually practising the test, and the problem could be definitely formulated in words. Then followed practice in listening to the test as played on the gramophone, and in calling out the answers together. An entire side of a record would if necessary be played through. The next step was to demonstrate the method of recording the answers. The record form was copied in large scale on the blackboard, and as the class, listening to the gramophone, called out the first few answers, the experimenter or one of the students wrote them down in their proper places on the record form. Finally, the instructions were once more repeated, and the test proper began. In each test the record was played twice over.

The specific procedures by which the different tests were demonstrated are briefly described as follows.

Test 1. *Sense of pitch.* The examiner simply played two successive notes of different pitch on the harmonium, and asked whether the second note was higher or lower than the first. There was no difficulty here except that of novelty, which repetition soon overcame.

Test 2. *Sense of intensity.* The examiner struck a note, and repeated it with a different intensity. The intensity was controlled by the use or non-use of the loud pedal. The class readily understood that they were to say whether the second note was weaker or stronger than the first.

Test 3. *Sense of time.* Three dots were made in a row on the blackboard, and numbered 1, 2, 3. The examiner asked whether the distance from dot 2 to dot 3 was longer or shorter than the distance from dot 1 to dot 2. This was repeated *ad lib.* with the second interval sometimes longer than the first, sometimes shorter. Then the problem was translated from visual into auditory terms, substituting taps with a pointer for dots, and the subjects judged whether the second time interval was longer or shorter than the first.

Test 4. *Sense of consonance.* The three criteria by which consonance was to be judged were written on the blackboard, thus:

GOOD	BAD
1. Go well together, agree.	1. Do not go well together, disagree.
2. Smooth.	2. Rough, harsh.
3. Sound almost like one note.	3. Do not sound at all like one note, but like two.

A consonant pair of notes, and a dissonant pair, were then played on the harmonium, and contrasted in respect of the first criterion, blending. The second criterion, smoothness, and the third, fusion, were illustrated in the same way. Attention was then drawn to the contrasting lists of criteria on the blackboard, and with these in view the class were exercised, first with the harmonium and then with the gramophone, in deciding whether the second of two combinations was better or worse than the first. The lists of criteria were left on the blackboard during the test.

There was considerable difficulty in making the nature of this test understood, and it is probable that in the case of many of the subjects the "cognitive limit" rather than the "physiological limit" ((1), p. 51) was measured.

Test 5. *Tonal memory.* The examiner wrote on the blackboard

1 2
1 2

and affixed some distinguishing mark, such as a dot or a cross, to one of the numbers in the second row. He then asked the class which number was different in the second row as compared with the first. Sometimes one number in the second row would be thus distinguished by a mark, sometimes the other. The length of the series of numbers was gradually increased from two to three, four, five and finally six numbers. Each time the class called out the number which was different in the second row. When the problem had been thoroughly understood in these visual terms, it was translated into auditory terms, by using series of notes of different pitch instead of numbers. Keeping in view the series of numbers on the blackboard, the class counted the notes silently as they were played, then called out the number of the note which was different the second time. Practice was given with series of from two to six notes, first on the harmonium, then on the gramophone. The two rows of numbers, of the appropriate length, were in view on the blackboard during the test.

This test, like the previous one, was difficult to make understood.

Test 6. *Sense of rhythm.* The examiner made five or six taps with a pointer on the table, the taps forming a definite rhythmical pattern; then in similar fashion he beat out the same or a different rhythm. The class readily grasped that they were to say whether the second rhythm was the same as or different from the first.

V. RESULTS.

Seashore provides grade norms for the interpretation of scores in the tests. He found a decided increase in score from school grade to school grade. This he attributes not to improvement in the musical capacities measured by the tests, but to non-musical factors incidentally measured, such as maturity of intelligence and range of information.

The students of the Alliance High School are grouped into five forms.

According to the official regulations, Form 1 does work nominally of the same standard as thirteen-year old pupils in England, and therefore of approximately the same standard as seventh grade pupils in the United States. This, it might be expected, would provide a basis of comparison, on which Form 1 would be equivalent to the American seventh grade, Form 2 to the American eighth grade, and so on.

In point of fact, however, there is no such increase in score from form to form as there is from grade to grade. The differences between the average scores of the forms are mostly small in amount and irregular in direction. Since one form, then, does as well on the whole as another, it is reasonable that the scores of all the students should be interpreted according to the same standard. The question arises, which standard? It was decided to use the norms for the American seventh grade, for the following reasons. First, forty-one, or nearly half of the students, are in Form 1, which corresponds to Grade 7; twenty-six are in Form 2, twenty-one in Form 3, one in Form 4 and one in Form 5. Secondly, since the tests were conducted only a month after the beginning of the school year, all the students were only at the beginning of their respective forms. The transmutation of scores into percentile ranks has therefore been made according to the American seventh grade norms.

Table II gives the mean and the standard deviation of the scores in each of the six tests. The column headed "Percentile rank of mean" gives, for each test, the percentile rank, on the American seventh grade scale, to which the African mean score corresponds. Since for some purposes it may be desirable to have data on a pure tribal group rather than on a mixture of tribes, the data are given for the largest tribal group, the sixty-seven Kikuyus, as well as for the school population as a whole.

Table II. *Mean scores, etc. of East African natives in the Seashore Measures of Musical Talent.*

	All tribes. $N=90$			Kikuyus only. $N=67$		
Test	M	σ	Percentile rank of mean	M	σ	Percentile rank of mean
1. Pitch	70·19	11·25	40	68·55	10·60	38
2. Intensity	88 56	9·10	88	87·03	9·90	80
3. Time	76·76	11 60	85	76·77	12·10	85
4. Consonance	58·47	7 60	25	59·31	7·30	28
5. Tonal memory	45·97	14·80	22	42·46	10·90	15
6. Rhythm	71·79	10·30	70	69·84	10·35	59

Seashore publishes frequency curves of the scores made by American fifth-grade children, eighth-grade children, and adults. The frequency curves of the African scores (not published for lack of space), yield the following points of comparison.

Test 1. *Sense of pitch.* The African curve resembles the rather flat curve of the American fifth grade, but runs somewhat higher, probably about where the sixth-grade curve would run. The African mean, 70, is in point of fact the sixth-grade median.

Test 2. *Sense of intensity.* The African curve closely resembles the negatively skewed curve for American adults. The African mean, 89, is the American adult 51 percentile.

Test 3. *Sense of time.* The African curve resembles the negatively skewed curve for American adults, but runs higher. The African mean of 77 is reached or exceeded by only 42 per cent. of American adults.

Test 4. *Sense of consonance.* The African frequency curve is, like the American fifth-grade curve, positively skewed, but the scores are considerably more piled up at the lower end of the scale. Only 38 per cent. of fifth-grade children fail to reach or exceed the African mean of 58.

Test 5. *Tonal memory.* The African curve somewhat resembles the very flat American fifth-grade curve, but is considerably more piled up at the lower end. The African mean of 46 is only the fifth grade 41 percentile.

Test 6. *Sense of rhythm.* The African frequency curve resembles the normal curves of the eighth grade and of adults, runs between them, and is flatter. The African mean of 72 exceeds the score of 35 per cent. of adults and 63 per cent. of eighth-grade children.

In two of the tests, those of the senses of consonance and of memory for tones, the frequency curves of the African scores very definitely rise at the lower end of the scale, in the region where the scores are little or not at all superior to those attainable by chance. This may quite possibly be due to a failure to understand the task in these tests on the part of some of the subjects. These two tests were certainly the most difficult to make understood; and, as will be seen later, it was the less intelligent subjects who made the lower scores.

The differences between the mean scores of the Kikuyus and those of the whole school population, of which, of course, the Kikuyus form the largest part, are slight.

On the assumption that the African scores may be evaluated by the American seventh-grade norms, Table II shows the Africans to be superior to the Americans in the senses of intensity, time and rhythm, and inferior in the sense of pitch, the sense of consonance and in memory for tones. If the average musical talent of the Americans be taken as normal, the average musical talent of the Africans will exhibit the profile shown in Fig. 1.

The senses of pitch, intensity and time are the basic capacities in musical talent, being psychological correlates of the physical attributes

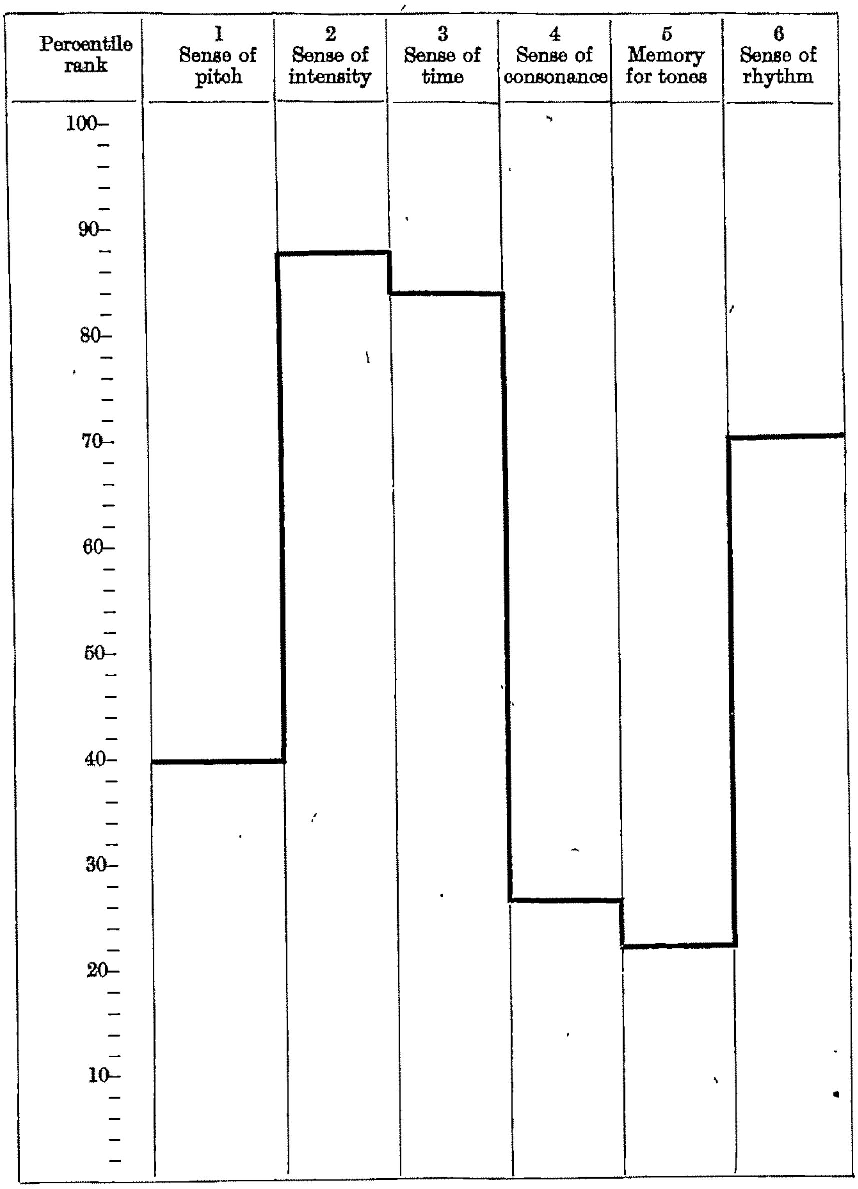

Fig. 1. Average musical talent chart of ninety East African natives.

of sound. The other three capacities are complex derivatives of these and other factors. The sense of consonance depends partly on the sense of pitch, so that if the sense of pitch be poor, as here, the sense of consonance will be likewise poor, even apart from a deficiency in the other elements in the sense of consonance. Memory for tones is obviously dependent, among other things, on hearing differences of tone in the first place, so that it can be no better than the sense of pitch, and may, as here, be poorer. The sense of rhythm stands in an analogous relation to the senses of intensity and of time; excellence in these senses augurs a good sense of rhythm, as here, though it does not guarantee it.

In so far as the tests measure what they purport to measure, we should expect a correlation between the African's musical talent profile and the characteristics of African music. Thus, in view of the highly developed rhythm which is such an outstanding feature of African music, it is not surprising to find the tests registering, in the Africans, superiority in the sense of rhythm, and excellence in the senses of time and intensity on which the sense of rhythm largely depends. Likewise, there is probably a causal connection between the poor sense of consonance indicated in the Africans by the tests, and the fact that African music, like all non-European music, is built on pure melody and not on harmony. The poor record of the Africans in the sense of pitch is somewhat surprising, to the writer at least. The successive notes in African melody seem often to involve quite small differences in pitch. Can it be that the test does not suit Africans as well as it suits Europeans? A melody has a different Gestalt from two contrasted notes. The European may be more efficient in dealing with the more analytical situation, in virtue of some non-musical capacity. The apparent deficiency in memory for tones, too, is surely somewhat surprising in view of that other outstanding characteristic of African music—antiphony. In African song, solo and chorus commonly alternate, the chorus taking up and repeating the part just sung, often extempore, by the soloist. This would seem to call for much the same kind of immediate memory as that measured by the test.

The best record in the series of tests was made by a Bantu Kavirondo boy, aged fifteen, in Form 2. His scores in tests 1 to 6 respectively were: 89, 93, 85, 83, 86, 85. Form 2, according to the school regulations, should be equivalent to the American eighth grade, and an American boy of fifteen would very probably be found in the eighth grade: the eighth grade norms may therefore be very fairly used for the comparison of this African with Americans in musical talent. He is found then to equal or excel, in sense of pitch, 97 per cent. of Americans of his own school standing; in sense of intensity, 95 per cent.; in sense of time, 98 per cent.; in sense of consonance, 99 per cent.; in memory for tones, 92 per cent.; and in sense of rhythm, 97 per cent.

As has been stated, ten students receive special instruction in music. They were selected by their own wish. It was found that only four of these students were among the ten individuals whom the tests would select as the most talented. Several members of the class were found to have quite poor records in the tests, and on enquiry the music instructor reported that these students were not making satisfactory progress. The principal of the school was accordingly enabled to withdraw these students from the class, and to replace them by others whom the tests showed to be more talented.

Seashore found, in each test, an increase in score with age and with school grade. He attributes the increase entirely to school progress, and not at all to age in itself. Our results would confirm his opinion that the abilities measured by the tests do not develop with age, within the age limits of our group (12 to 24) at least. The coefficients of correlation of age with score in each of the tests is given in Table III.

Table III. *Coefficients of correlation between musical talent scores and age of ninety East African natives.*

Test	r	PE_r
1. Sense of pitch	+0·003	±0·07
2. Sense of intensity	−0·07	±0·07
3. Sense of time	+0·05	±0·07
4. Sense of consonance	−0·07	±0·07
5. Tonal memory	+0·13	±0·07
6. Sense of rhythm	−0·10	±0·07

Seashore and others have found positive but very small correlations between the measures of musical talent and measures of intelligence. The subjects of this experiment were ranked in order of intelligence by the four European members of the school staff. The ranks assigned by each member were converted into scores on a linear scale, and the four series of scores were averaged to give a composite intelligence score for each pupil. The reliability coefficient of these composite intelligence scores was 0·77. These scores were correlated with the scores in each of the six tests of musical capacity, with the results shown in Table IV. The coefficients have been corrected for attenuation.

It will be seen that between four of the tests and intelligence there is no statistically significant correlation. The other two tests, those of the sense of consonance and of memory for tones, are positively correlated with intelligence. This may indicate that these two capacities in Africans are correlated; or that the tests of these two capacities, when applied to Africans, are not entirely valid, being in some degree measures of intelligence; or, perhaps most probably, that the methods of administering these tests were not successful in making the test problems understood equally well by the more and the less intelligent. In the latter event, merely the 'cognitive limit' rather than the 'physiological limit' would be measured, in the case of the less intelligent subjects. Failure to make the tests thoroughly understood would also account, to some extent at least, for the apparent inferiority of the Africans to Americans in these two capacities. The data on the reliability of the measures of musical talent with this group of subjects are given in Table V.

Table IV. *Coefficients of correlation between measures of musical talent and of intelligence, of ninety East African natives.*

Test	r	PE_r
1. Sense of pitch	+0·25	±0·07
2. Sense of intensity	+0·16	±0·07
3. Sense of time	−0·04	±0·07
4. Sense of consonance	+0·59	±0·05
5. Memory for tones	+0·45	±0·06
6. Sense of rhythm	+0·17	±0·07

Table V. *Data on the reliability of measures of musical talent with ninety East African natives.*

Test	Reliability coefficient: Of one block	Reliability coefficient: Of two blocks	Minimum number of blocks required
1. Sense of pitch	0·72	0·84	2
2. Sense of intensity	0·93	0·96	1
3. Sense of time	0·78	0·88	2
4. Sense of consonance	0·40	0·57	6
5. Tonal memory	0·88	0·94	1
6. Sense of rhythm	0·74	0·85	2

One block of trials consists of the playing of both sides of a test disc. As has been stated, two blocks were given in each test. The first row of figures in Table V shows the coefficient of correlation between the scores in the first block and those in the second block. The second row, which is derived from the first by Spearman's prophecy formula, gives the reliability coefficients of the tests as actually conducted. If 0·80 be taken as the minimum below which a reliability coefficient, to be satisfactory, should not fall, it will be seen that all the tests, except one, were sufficiently reliable as actually conducted. Indeed, one block would have sufficed for tests 2 and 5. Test 4 (Sense of Consonance) was not reliable, and, according to the Spearman formula, six blocks would have been necessary to make it so; a better method of administering the test might, however, achieve the same result with fewer blocks.

VI. SUMMARY.

1. Ninety natives of East Africa were tested with the Seashore Measures of Musical Talent.
2. Special methods of administering the tests had to be devised.
3. The test scores of the Africans were compared with those of American seventh-grade children, these being taken as of approximately equivalent school standing. The Africans were superior to the Americans in the senses of intensity, time and rhythm, in that order; inferior in the sense of pitch, the sense of consonance and memory for tones, in order of increasing inferiority.
4. An African boy of about fifteen excelled over 90 per cent. of American children of approximately his own school standing in each of the six capacities.
5. There was no relation between any of the six capacities and age.
6. Four of the tests were uncorrelated with intelligence. The tests of the sense of consonance and of memory for tones were positively correlated with intelligence, probably owing to the nature of the tasks being difficult to understand.
7. All the tests, except that of the sense of consonance, were adequately reliable as actually administered.

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(*Manuscript received* 4 *October*, 1931.)

SOME OBSERVATIONS ON IMMEDIATE COLOUR MEMORY[1].

By MARY COLLINS.

(From the George Combe Psychological Laboratory, Edinburgh.)

The aims of this investigation were, on the one hand, to discover if individuals varied in their immediate memory for colours, and if so, to what extent, and, on the other hand, to ascertain if and under what conditions colours could be learned and memorized in the same way as other forms of material.

The experiment was carried out as follows. The observer was seated in front of a spectrometer (in this case the Drever-Hilger Spectrometer), and at a given signal looked through the eye-piece where a homogeneous field of a particular colour was visible. This was viewed for five seconds, after which a sheet of grey paper was fixated for fifteen seconds. During this time the wave-length on the spectrometer was changed by the experimenter. At the end of the fifteen seconds, the observer was asked to reproduce the original colour, which he attempted to do by himself moving the drum of the spectrometer. The amount of displacement made by the experimenter and the observer's estimates were noted each time. Four readings were taken daily for each colour chosen—two readings in which the displacement made by the experimenter was in the direction of a shorter wave-length than the standard chosen, two in which the displacement by the experimenter was in the direction of a longer wave-length.

The colours chosen were four in number. Three of them, the red, the green and the yellow, had already been used by the experimenter in investigations with the Rayleigh equation, and a fourth, a blue, was added. The wave-lengths of the colours selected were red (λ 670), yellow (λ 588), green (λ 535) and blue (λ 460·9).

The observers were six in number. Five of these were all trained psychologists; the sixth was quite untrained and much younger than any of the others. The experiment lasted over a number of weeks, and for some of the observers over 100 readings were taken for each colour.

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[1] A paper read before the Psychology Section of the British Association for the Advancement of Science at London, 1931.

RESULTS.

I. The first main result which emerges, and one perhaps to be expected, is the great individual differences which are revealed. Some of the observers reveal a distinct aptitude for colour learning, and some only for particular colours. One observer could tell in the case of the yellow the minutest change in wave-length. No learning was required. He could reproduce this colour very accurately each time, and his results for this colour show uniformly very small deviations from the standard. In Fig. 1 appears in graphical form the readings given by the various observers for the yellow. These have been averaged in groups of five to bring them together on one diagram. Each reading is therefore the average of five separate readings. The two sets of readings for the different displacements have been kept separate. The standard line of 588 is shown in each case.

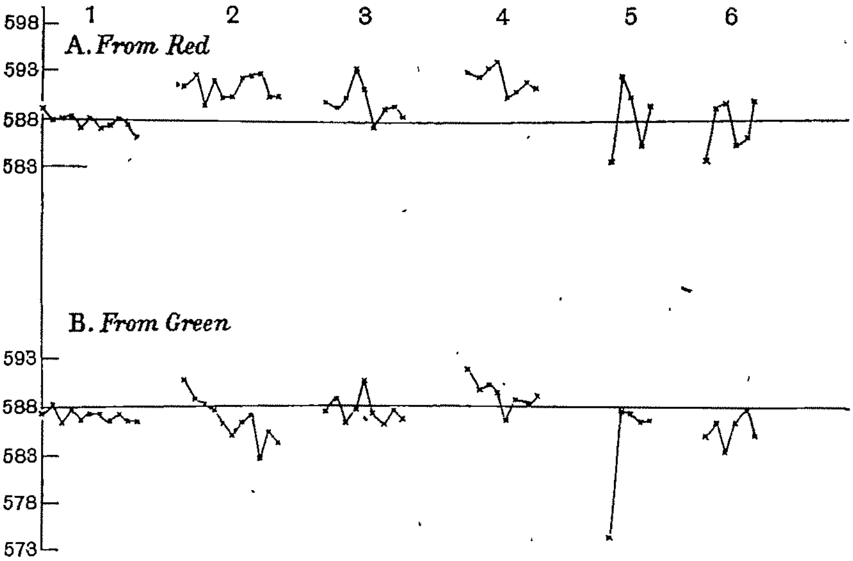

Fig. 1. Yellow, λ 588. Each point is the average of five readings.

The first observer is consistently the most careful subject, and gives the best readings throughout. For the hundred odd readings there is very little scatter, the graph being practically a straight line. By him the yellow was much more accurately memorized from the very beginning than by any of the others. The youngest subject, observer 6, it will be noticed, also shows very good results, and keeps fairly consistent. The fifth observer makes a bad start, but soon recovers. For all the subjects, although they vary in their reproductions, there are no extreme deviations. This is probably because of the rapid change of hue with wavelength at this particular region of the spectrum.

Fig. 2 shows the results for the blue. Again the first subject has few errors and reproduces well. He does not make such a good start but quickly recovers. With the other subjects, on the whole, there is more deviation than in the case of the yellow, and the results are slightly more erratic. The second subject developed an antagonistic attitude towards the blue which is reflected in her results.

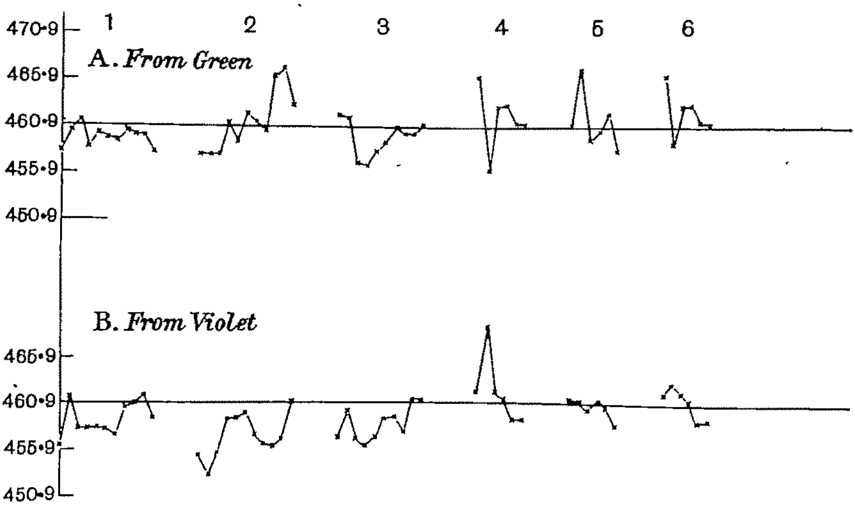

Fig. 2. Blue, λ 460·9. Each point is the average of five readings.

These individual differences appear in a similar manner with the other two colours, the red and the green (*vide* Figs. 3 and 4).

II. The second main result which emerges is the differences in difficulty inherent in the colours themselves. The curves of the blue and yellow are, as already shown, practically linear in form, the yellow more so than the blue, but the curves of the red and green are markedly different. Fig. 3 shows the readings for the red. The greater deviations which occur in this case are quite apparent, as much as 10 to 20$\mu\mu$. Some of the results do not approach the standard line at all, and the errors are considerably increased. In the case of the red, as compared with the yellow and the blue, greater learning is required before the colour is mastered, and even after an hundred odd trials it is doubtful if the colour has been learned, for the curves in no wise resemble plateaus.

All one can say is that the errors are on the whole decreasing, and that the reproductions approach nearer to the standard. But for most the learning is still incomplete. There is a tendency throughout to reproduce a red redder than the standard. Even when the start is made from the yellow end, the general tendency is to take out too much yellow, to pass the standard line, and to go over into the more extreme red. Occasionally, over-compensation for this error seems to be taking place.

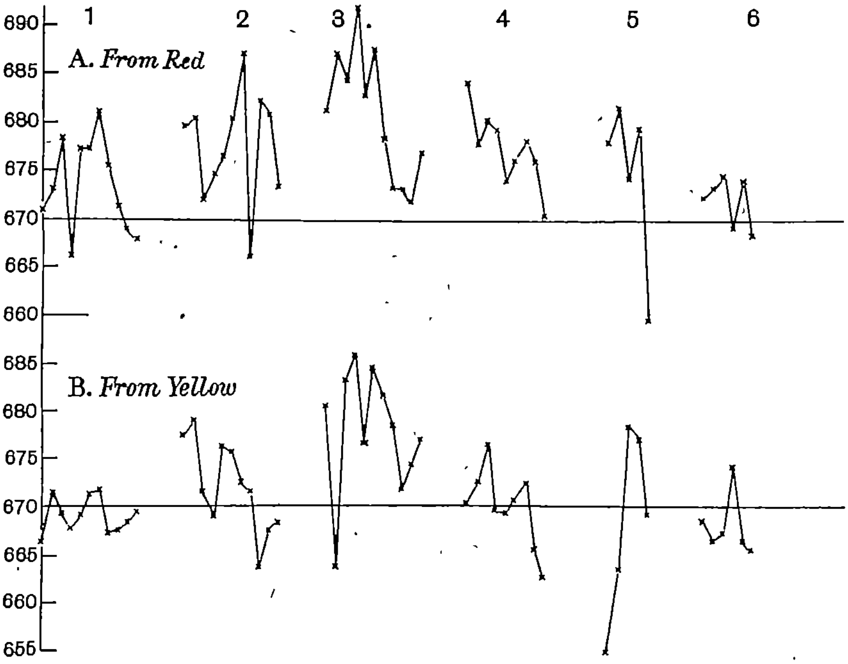

Fig. 3. Red, λ 670. Each point is the average of five readings.

The green (Fig. 4) also is characterized by large deviations. Subjectively this colour appeared the most difficult of all to learn, much more difficult than the red, although objectively the actual deviations and errors made are no greater. The tendency here again is to take out too much yellow; the results, it will be observed, are all towards the shorter wave-length. Even when starting from yellow, too much yellow is removed; and this tendency could not be successfully compensated even when the results were known. The second observer shows an attempt at compensation which has been successful to begin with, but over-compensation has entered in. From the blue end, the results are a little worse. Some of the observers never reach the standard line at all even with the full knowledge of their results. It was a most elusive colour to match and reproduce. Even when the observer felt fully confident of his placing, the results showed considerable error.

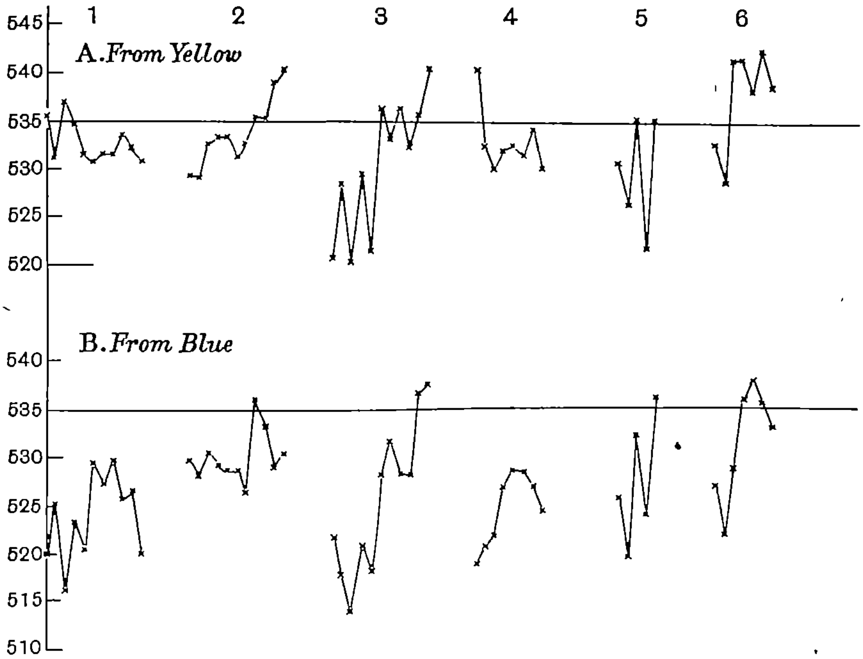

Fig. 4. Green, λ 535. Each point is the average of five readings.

These four colours seem to sort themselves out into two groups so far as learning is concerned, blue and yellow form one group, red and green the other. The first group is easily learned, the second group presents greater difficulty.

The averages for these four colours for all the observers bring this out clearly (Table I).

Table I. *Average of the deviations from the standard for all the trials.*

	Observers					
	1	2	3	4	5	6
Red	5·9	8·0	12·7	6·6	14·0	6·1
Green	7·4	6·2	9·3	7·6	12·0	7·5
Blue	2·3	3·7	2·5	2·4	2·6	2·8
Yellow	1·3	3·0	2·0	3·2	4·6	3·5

Why the yellow and the blue should be easier to learn than the red and the green is not easy to determine. The yellow is not a pure yellow, but has a slightly reddish tinge in it; the red is a yellowish red, the green a yellowish green, though the blue perhaps might be termed a pure blue. It is not therefore a difference as between primary and compound colours.

Two supplementary experiments gave illuminating results. A blue of 486 $\mu\mu$ proved very easy to remember, the average of ten readings being 485·2 ± 2·3. A green of wave-length 500 $\mu\mu$ also proved a very easy colour to remember. The average of a number of readings worked out at 501·4 ± 2·3. These results seem to point to the fact that the particular green chosen in the main experiment λ 535, and the particular red chosen λ 670 possess some quality inherent in themselves which make them difficult to recognize again and almost impossible to reproduce. This suggests that different wave-lengths of the spectrum are not equal as regards retentivity.

These results, of course, cannot be taken as general. They only hold for the special conditions of the experiment, and may differ for other and different conditions.

III. The third main result is as regards the actual learning of the colours. That learning did take place is evident from the objective results obtained. Table II shows the results of the learning for two of the colours. The average error of the first ten readings, and the average error of the last ten readings have been calculated for three of the observers. We may take these results as being representative of the other observers. The averages of the deviations from the standard for the first ten and the last ten readings have also been calculated, and these appear within the brackets.

Table II. *Average error of first ten and last ten readings.*

Figures in brackets show average of the deviations from the standard of first ten and last ten readings regardless of sign.

	Yellow		Green	
Observers	First ten	Last ten	First ten	Last ten
No. 1	+0·5 (1·9)	−1·0 (1·2)	− 1·6 (4·1)	− 3·4 (4·7)
	−0·3 (1·5)	−1·5 (1·5)	−12·4 (12·1)	−11·7 (6·7)
2	+4·2 (4·3)	+1·7 (3·2)	− 5·9 (5·8)	+ 4·7 (6·3)
	+1·7 (2·3)	−3·3 (3·2)	− 6·1 (8·5)	− 5·3 (6·0)
3	+1·7 (2·2)	+0·5 (1·9)	−10·4 (11·8)	+ 3·8 (5·5)
	+0·1 (1·1)	−0·8 (1·6)	−15·6 (16·9)	+ 2·1 (5·2)

In the case of the yellow, for the first observer, the average error of the first ten readings is + 0·5, for the last ten − 1·0. These are the errors of the reproductions when starting from the red end of the spectrum. Starting from the other side, from the green, his errors are − 0·3 for the first ten, − 1·5 for the last ten. No learning is required in this case as the colour was accurately reproduced from the beginning. This raises the question as to whether an absolute sense for colour exists analogous to absolute pitch in hearing? It does seem to do so for this particular case. The average of the deviations also show constancy, 1·9, 1·2, 1·5, 1·5. The third observer is equally good, with small errors and deviations. The second observer is not quite so good.

In the case of the green there are bigger errors with learning in some cases but not in others. The first observer's average error from the one end is small, − 1·6 and − 3·4, but the average of the deviations is larger than in the case of the yellow. From the other end the errors are very large, − 12·4 to − 11·7, with average deviations of 12·1 and 6·7. There is practically no learning evident here. The second observer gives better results and keeps fairly constant, but the errors are again greater than in the case of the yellow. The third observer shows greatest learning of all, reducing his average error from − 10·4 to + 3·8, and from − 15·6 to + 2·1. These are not comparable, however, to his results with the yellow, but they do show clearly the effects of practice.

These results again bring out the difference between the two sets of colours, blue and yellow on the one hand, for the results of the blue are very similar to those of the yellow, and red and green on the other hand, for the red results resemble those of the green.

IV. The testing of the learning at the conclusion of the experiment again emphasizes these differences. Reproductions were taken by all the observers on ten consecutive days. The results appear in Table III which shows the average placing over the ten days.

Table III. *Testing of learning on ten consecutive days for all the observers (average of deviations from standard).*

Observers	Yellow	Blue	Red	Green
No. 1	2·8	2·6	3·6	8·2
2	5·4	11·5	6·0	11·4
3	2·9	1·4	9·6	6·9
4	3·7	6·2	7·1	6·4
5	4·2	6·9	4·8	8·1
6	2·5	2·9	13·0	17·1

On the whole, the yellow and the blue are better retained than the red and the green. The one exception in the case of the blue (11·5) is explained by the antagonistic attitude, already mentioned, developed by this observer. She set out with the idea that the blue was rather easy and did not matter much, and was more interested in mastering the red and the green. About the middle of the experiment she suddenly realized that the blue was proving a little more elusive than she anticipated, and a slight feeling of irritation at this finding gradually developed into a dislike for this particular blue. All through, her results for this particular colour are much higher than they should be.

The youngest observer, who has done rather well throughout, fails rather badly in the red and green when the learning is tested. His blue and yellow are, however, still very good.

The first observer developed a pure memory of the yellow. Tested two months later, without seeing the colour in the interval, his error was 0·1 $\mu\mu$, whereas the errors for all the other colours were much greater, ranging from 5 to 12 $\mu\mu$. The third observer also developed a pure memory for the yellow. The red and green for him, however, were not memories, but distances away from the yellow in the case of the red, and from the blue-green in the case of the green. In other words, a mnemonic system was apt to develop with these two colours.

V. The final point which emerges with respect to this investigation is the relation of the reproductions made to the thresholds for the same colours. The threshold for each colour was obtained for each observer by the method of limits and serial groups. The ratio of the average of the total deviations to each threshold was then calculated for each colour and observer. The results are shown in Table IV. The results of observer 6 are omitted as they are not reliable.

Table IV. *Ratio of total average deviation from standard to threshold.*

Observers	Yellow	Blue	Red	Green
No. 1	1	1·8	2·4	6·0
2	2	1·5	4·0	4·5
3	1·6	4·0	6·0	4·4
4	1·1	0·04	1·8	2·2
5	3·0	3·0	7·0	3·6

In the case of the yellow, for observer 1 the average deviation corresponds to the threshold; i.e. for observer 2 the average deviation is twice as great as the threshold; i.e. for observer 3 the average deviation is 1·6 times as great as the threshold and so on. Blue, red and green similarly.

On the whole, the ratios for the red and green are larger than those for the yellow and blue.

Once again, therefore, this emphasizes the difference between the two sets of colours. Whereas, on the whole, the errors of reproduction fall within or nearly within the thresholds for the yellow and the blue, they are far from falling within the thresholds for the red and the green.

König and Dieterici found three maxima in the spectrum as regards maximum discrimination sensibility for hue. One of these was at λ 590 which corresponds with our λ 588, and this would explain the nature of the results obtained at this wave-length. Conversely, one of the minima found was at λ 540, near our green. Steindler's corresponding minimum was actually placed at λ 535.

This difference between the two sets of colours is the most interesting result which emerges from this investigation. Whether other parts of the spectrum will act similarly and present as great difficulty as the red λ 670 and green λ 535 remains for further investigation.

THEORETICAL AND PRACTICAL ASPECTS OF INTELLIGENCE.

BY HENRI PIÉRON.

IN its current usage, the term intelligence has an essentially practical significance, because to say that a man or child is intelligent is to make a "judgment of value," that is, to rank him categorically according to a certain point of view above the average. But is this concept of intelligence actually well defined and unified? We have here a practical problem of great importance. Is there a general and universal superiority of intelligence which is such that success in given tests, forming a classification of a group in terms of their intelligence level, permits the prediction of success in other tests given for the same purpose, the rank obtained by the different individuals remaining practically the same whatever the tests may be?

Besides, parallel to this practical problem the theoretical question presents itself in a manner analogous, at least in appearance. For, putting to one side the question of a faculty of intelligence, one continues to envisage the existence of a function 'intelligence' as constituting a true entity specifically defined, or one discusses the validity of a unified factor representing a 'general intelligence' in the mental life.

What help is given us by the present-day psychological researches on this double problem?

Let us first consider this problem from a practical point of view. When one is not content to employ subjective judgments which do not easily permit coherent classifications of different individuals, one calls upon tests in order to determine an intelligence level.

But as so-called tests of intelligence one often uses tests which cannot be regarded as satisfactory; certain of them, because of their scholastic nature, give too large a place to educational differences (arithmetic problems, for instance), so that individuals actually inferior to others but better trained are more successful; other tests aimed at determining with more precision the mental development level of children, call upon processes of perception, attention and memory, which give no accurate evidence with regard to the practical capacities designated by the term 'intelligence,' or ability to solve new problems and to resolve difficulties for which there is no learned formula.

Satisfactory tests can then consist only of problems to be solved for which there has been no educational training. There are large numbers of such tests which present problems of a varied nature. The subject is required to extricate the moral of a fable, discover the significance of a proverb, find the equivalent of a verbal symbol, search out absurdities, recognize a serial relationship, arrive at a correct conclusion proceeding from premises by means of logical reasoning, etc.

Moreover, many other problems different from those for which useable tests have already been built, present themselves in everyday life. But can we, within the limitations of these intelligence tests, adopt any sort of examination to permit a general scaling of individuals on the basis of intelligence? Unfortunately the facts answer this question very clearly in the negative, because the success of a given individual varies as a function of the nature of the problem.

My contribution has been an attempt to arrange a fairly homogeneous battery of tests for use in vocational guidance.

As a provisional division, the value of which I do not wish to exaggerate, I have been led to differentiate according to the nature of the problem, a logical type of intelligence, a preponderantly verbal type, a type principally numerical in character (completion series), and a general type corresponding roughly to common sense.

By utilizing results obtained during the standardization of this psychological battery on Parisian school children[1] it was possible, thanks to the co-operation of my collaborators, Mr A. Fessard and Miss J. Monnin, to determine the correlations of the scores of a large number of boys and girls for the tests falling into the above-mentioned four categories[2].

The Pearson coefficient of correlation based on the results of 564 boys and 389 girls (from 11 to 15 years of age belonging to the "Cours moyen et supérieur des écoles primaires," who have already obtained their "Certificat d'études) are as follows:

	Verbal intelligence		Logical intelligence		Numerical intelligence	
Intelligence	Boys	Girls	Boys	Girls	Boys	Girls
General	0·225 ± 0·027	0·284 ± 0·031	0·470 ± 0·022	0·408 ± 0·029	0·190 ± 0·027	0·012 ± 0·034
Verbal	—	—	0·260 ± 0·026	0·227 ± 0·032	0·190 ± 0·027	0·180 ± 0·033
Logical	—	—	—	—	0·140 ± 0·028	0·059 ± 0·034

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[1] Cf. M. and Mme Henri Piéron, "Instructions pour l'emploi de la fiche psychologique d'orientation professionnelle," *Bull. de l'Inst. Nat. d'O. P.* 1930, II, 7, 161.

[2] Cf. A. Fessard, Jeanne Monnin and H. Piéron, "Quelques données sur les indépendances fonctionnelles révélées par les épreuves psychotechniques" (7me Conf. Inter. de Psychotechnique), *Bull. de l'Inst. Nat. d'O. P.*, Oct. 1931, III, 8.

These correlation coefficients, based on a large population of subjects, are of similar magnitude for both sexes except those expressing the relationship between numerical intelligence and general intelligence on the one hand, and logical intelligence on the other, which are almost zero for the girls.

The coefficients are all positive but low, except for those expressing the relationship between general and logical intelligence, and do not permit prediction of success in a group of tests from scores on another group. In fact, when the correlation between two series of scores is about 0·20, 49 per cent. of the individuals placed above the mean according to one test, fall below the mean on the other, as against a chance expectancy of 50 per cent.

For practical purposes, one may therefore say that it is not possible to predict from one group of tests to the other, that, for example, success in discovering the law of a numerical series does not at all indicate an equivalent capacity for discovering the moral of a fable, for finding analogies, drawing the conclusion from a syllogism, etc.

Moreover, the technical organization of the test material itself affords a device which can bring into evidence an aspect of the practical capacity which we call intelligence, which is sufficiently characteristic to afford a measure of success depending upon this factor.

That is to say, in certain tests the subject is asked to choose from several alternatives the correct response, and it can be held that *understanding* is sufficient to bring success; in others, one has to find the correct solution which is not given, thus necessitating *invention*; finally, the ability to *criticize* sometimes occupies the most important position as when the subject is asked to indicate an absurdity, strike out of a series a nonconforming element, etc.

The nature of the examination therefore allows, on the part of the mind confronted by a problem, an emphasis on a particular one of these three types of attack such as the comprehension of the nature of the task, the finding of a solution or the criticism of a possible solution in terms of the given conditions of the problem.

This permits a demonstration of the fact that individuals can vary in capacity for one or another of these forms of intelligent activity. Even though the results obtained with A. Fessard and J. Monnin cannot be regarded as final, though new researches in this direction have been undertaken, I shall give, nevertheless, the correlation coefficients obtained between the results of tests groups on the basis of these three above differentiated aspects of ability, this time independent of the literal nature of the questions themselves:

	Criticism		Invention	
	Boys	Girls	Boys	Girls
Comprehension	0·310 ± 0·026	0·203 ± 0·032	0·270 ± 0·026	0·359 ± 0·030
Criticism	—	—	0·310 ± 0·025	0·283 ± 0·028

The coefficients this time prove to be a little higher than the preceding ones but do not yet permit a practical prediction.

With regard to psychotechnical applications it appears to be necessary no longer to consider one 'intelligence' but several aspects or multiple forms of it; therefore, in order to characterize an individual, it would be necessary to trace his profile in as detailed a manner as possible, giving place to the principal types of problems which he is called upon to solve in ordinary life, distinguishing at the same time the phases of comprehension, invention or criticism which intervene unequally according to the manner in which the problems are actually presented by various professional activities.

From the theoretical point of view it is well known that the relative independence of performance, giving evidence of the play of different multiple factors in the execution of the task, leaves untouched the problem of a common element which represents the fundamental functioning unity, the rôle of which is masked by that of the united action of the other factors.

The conclusion indicated by the low magnitude of the correlations, is that the possible importance of a common factor is really much less than that of the variable elements taken together.

I have not sufficient competency to take a part in the mathematical discussion of the statistical deductions from the experimental facts of Spearman's penetrating studies on the existence of a common factor; moreover tetrad differences computed from the experimental data in the elaboration of the above cited results, do not lend decisive support to either point of view.

But, if we admit the existence of a common factor, what might its nature be?

It proceeds from the facts already accumulated that the common factor is not limited to intelligence tests alone, but has a very general character and is involved in all mental tasks. It is probably an element of capacity showing itself in the most diverse functions as, in tests of attention, memory, perception, etc. The study of the correlations between various types of these tests which one might consider to measure a particular function, shows, as in the case of intelligence tests, so great an independence, that it is impossible to retain the idea of specific functioning in mental life; a conclusion which proceeds from the work done on the memory tests of the mental test battery with the help of A. Fessard and J. Monnin in which the intercorrelations proved to be practically zero.

There should be then in mental function—cerebral activity—whatever may be its varied manifestations, a common substrate which should always intervene as a factor in ability.

But can one say in a positive fashion that this factor is truly a unit? If the statistical deductions of Spearman are correct, we are concerned with an element which remains the same in performing different tasks, a factor the constance of which is in opposition to the variability of those elements, present or absent, which play a more or less important rôle. But an equal participation of a practically constant common element, is compatible with a complexity of this latter.

This common element in the execution of different mental tasks is, without doubt, the functioning organ (the cerebral apparatus) which remains about the same from one moment to another in a given individual, but which differs greatly from one individual to another.

However the cerebral apparatus is a complex ensemble and may differ between individuals in many ways in its quantitative characteristics (number and distribution of the cortical cells, and above all of the associative tracts), and also in its qualitative characteristics.

Particularly when regarded from this last point of view, it seems to me probable that individual divergences in the common basis symbolically represented by the G factor are of the greatest importance. What is the rôle played by those aspects of cellular metabolism of which one aspect is expressed by the notion of an individual and characteristic 'quickness of the neurone,' the 'chronaxie'? What is the rôle of the process of regulation acting through a liberation of nervous energy made possible by means of the interdependence between the cortical neurones and those neurones of the base of the brain which belong to the vegetative system and underlie a fundamental play of biological tendencies? What is the rôle of endocrine regulation? This is what the researches of the future will, perhaps, determine.

But one can now affirm that there is not, from a theoretical point of view, a specific function of intelligence, that there is not even a mental entity which merits the term intelligence, because the substrate of mental function common to all its manifestations is itself an extremely complicated system.

That biological unity on to which we project divisions corresponding to the diversity of our points of view and of our practical preoccupations cannot be reduced to isolatable elements which can be individually examined, a fact which renders difficult the psychotechnical analysis of individual capacities.

The present-day researches in applied psychology clearly demonstrate the futility of theoretical divisions of mental life into abstract entities, and illustrate also our obligation to particularize to the extreme the evaluations of functional aptitudes of which each one can have but one strictly limited meaning and application. We have no longer the right to differentiate in a definite manner between individuals of high and low intelligence, thus establishing a unique hierarchy. But, on the contrary, a hierarchy can only assume meaning and value when it is envisaged from a very particular and truly definite point of view.

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ON THE UNIQUENESS OF THE FACTOR G FOR GENERAL INTELLIGENCE.

BY J. O. IRWIN, M.A., D.SC.

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E. B. WILSON (1) has shown that if we have examined n candidates in k tests and we take n mutually perpendicular axes and refer the marks of the students in the tests to these axes, we can represent the performance of all the students in any one test by a vector **a** (whose direction cosines will be $a_1, a_2, \ldots, a_n$) in this n-dimensional space.

If we refer the marks to their mean and express them in terms of their standard deviation as a unit, we have $\Sigma a^2 = 1$, and **a** may be regarded as a unit vector.

We thus have k unit vectors **a**, **b**, **c**, etc. representing the performance of the students in the k tests. Since they are all unit vectors we may regard their end points as lying on a sphere the dimensions of whose surface are $(n - 1)$. The results of the tests are therefore represented by k points on a unit sphere whose surface is of $(n - 1)$ dimensions.

A spherical triangle always has an orthocentre, but a spherical tetrahedron or higher order tetrahedron[1] has not necessarily an orthocentre.

Wilson, however, shows that if the system is hierarchical: (i) any arc of the spherical figure formed by the k points is at right angles to any other arc which has not a common point with it; (ii) that the spherical tetrahedron (or the spherical k point, as it may be termed) formed by the k points has an orthocentre.

Conversely, if the tetrahedron has an orthocentre the system is hierarchical.

Wilson then goes on to show that g is a unit vector consisting of two components, one of which lies along the orthocentral axis and is determinate in magnitude; the other portion is determinate in magnitude but indeterminate in direction, since it can be any vector at right angles to the 1st in a space of $n-k-1$ dimensions which is at right angles to the space of $k+1$ dimensions containing the original k vectors and the vector which is equally inclined to all the co-ordinate axes.

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[1] Since each face is always a *triangle*, the term 'tetrahedron' has been used in a generalized sense in preference to 'polyhedron.'

Wilson thus finds

$$\mathbf{g} = \mathbf{r}_0 + \mathbf{v} \qquad \text{......(1)},$$

where $\mathbf{r}_0$ is the portion that lies along the orthocentral axis and is given by

$$\mathbf{r}_0 = \sqrt{\frac{\mathbf{b}.\mathbf{c}}{(\mathbf{a}.\mathbf{b})\,(\mathbf{a}.\mathbf{c})}}\left(\mathbf{a} - \frac{\mathbf{a}'}{\mathbf{a}.\mathbf{a}'}\right) + \sqrt{\frac{(\mathbf{a}.\mathbf{b})\,(\mathbf{a}.\mathbf{c})}{(\mathbf{b}.\mathbf{c})}}\,\frac{\mathbf{a}'}{(\mathbf{a}.\mathbf{a}')} \qquad \text{......(2)},$$

or, of course, to similar expressions in **b**, **c**, **d**, etc....

Here $\mathbf{a}'$, $\mathbf{b}'$, $\mathbf{c}'$, are the unit vectors which join the origin to a', b', c' which is the polar figure of a, b, c, ...[1].

On the other hand, Piaggio (2) finds

$$g = \kappa^2 t + \kappa i \qquad \text{......(3)},$$

where

$$t = S\left(\frac{\mu_a a}{\mu_a^2 - 1}\right),$$

$$\kappa^{-2} = 1 + S\,(1/\mu_a^2 - 1),$$

t being the linear function of the marks in the individual tests which correlates most highly with g and i a variable uncorrelated with $a, b, c, d, \ldots$.

Here

$$\mu_a = \left\{\frac{r_{bc}}{r_{ab}r_{ac}}\right\}^{\frac{1}{2}} = \left\{\frac{(\mathbf{b}.\mathbf{c})}{(\mathbf{a}.\mathbf{b})\,(\mathbf{a}.\mathbf{c})}\right\}^{\frac{1}{2}} = \frac{1}{r_{ag}} \qquad \text{......(4)},$$

κ is so chosen that the S.D. of i is unity; it easily follows that the S.D. of t is $\sqrt{1-\kappa^2}/\kappa^2$.

We can of course represent g, t and i by vectors in Wilson's n-space so that we have

$$\mathbf{g} = \kappa^2\mathbf{t} + \kappa\mathbf{i} \qquad \text{......(5)}.$$

It is the object of this paper to show that

$$\mathbf{r}_0 = \kappa^2\mathbf{t} \qquad \text{......(6)},$$

whence of course

$$\mathbf{v} = \kappa\mathbf{i} \qquad \text{......(6 \textit{bis})},$$

or, in other words, that Wilson's and Piaggio's solutions are really the same.

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[1] I have amended Wilson's expression in one particular. Wilson inadvertently takes $(\mathbf{a}\mathbf{a}') = 1$ which could not happen unless **a** coincided with $\mathbf{a}'$ which by hypothesis it does not.

If it is true that $\mathbf{r}_0 = \kappa^2 \mathbf{t}$,

we have
$$\mu_a \mathbf{a} - \frac{\mu_a^2 - 1}{\mu_a} \frac{\mathbf{a}'}{(\mathbf{a}.\mathbf{a}')} = \kappa^2 \mathbf{t}.$$

With similar expressions in b, b', c, c', etc. or

$$\left.\begin{array}{l} \dfrac{\mu_a^2 - 1}{\mu_a} \dfrac{\mathbf{a}'}{(\mathbf{a}.\mathbf{a}')} = \mu_a \mathbf{a} - \kappa^2 \mathbf{t} \\ \dfrac{\mu_b^2 - 1}{\mu_b} \dfrac{\mathbf{b}'}{(\mathbf{b}.\mathbf{b}')} = \mu_b \mathbf{b} - \kappa^2 \mathbf{t} \\ \dots\dots\dots\dots\dots\dots \end{array}\right\} \qquad \dots\dots(7).$$

The method of proof that we shall adopt is to find the equations for $\mathbf{a}'$, $\mathbf{b}'$, $\mathbf{c}'$, ... in terms $\mathbf{a}$, $\mathbf{b}$, $\mathbf{c}$, ... which follow in virtue of the fact that a', b', c', ... a, b, c, ... are reciprocal figures, to show that they have only one solution and that this solution is given by the equations (7).

Since $\mathbf{a}'$ must be at right angles to $\mathbf{b}'$, $\mathbf{c}'$, $\mathbf{d}'$, etc., and so on, we have the $(k-1)$ equations

$$\left.\begin{array}{l} (\mathbf{a}'.\mathbf{b}) = 0 \\ (\mathbf{a}'.\mathbf{c}) = 0 \\ \dots\dots\dots \\ (\mathbf{a}'.\mathbf{p}) = 0 \end{array}\right\} \qquad \dots\dots(8),$$

p being the vector corresponding to the kth test.

But the point a' must also lie in the k space through the origin and the k points a, b, c, d, ..., p, which gives us $(n-k)$ conditions which may be expressed by the determinants

$$\underset{(r = 1, 2, 3, \dots, (n-k))}{\Delta_r} = \begin{vmatrix} a'_r & a'_{r+1} & a'_{r+2} & \cdots & a'_{r+k} \\ a_r & a_{r+1} & a_{r+2} & \cdots & a_{r+k} \\ b_r & b_{r+1} & b_{r+2} & \cdots & b_{r+k} \\ \dots & \dots & \dots & \dots & \dots \\ p_r & p_{r+1} & p_{r+2} & \cdots & p_{r+k} \end{vmatrix} = 0 \qquad \dots\dots(9).$$

Equations (8) and (9) give $(n-1)$ equations for $a'_1, a'_2, \dots, a'_n$ which will determine their ratios uniquely and their absolute magnitudes are then determined from $\Sigma\, a_r^2 = 1$.

Thus $\mathbf{a}'$ is uniquely determined from these equations.

It will be shown that the expression for $\mathbf{a}'$ in (7) satisfies these equations.

From (7) we have at once

$$\frac{\mu_a{}^2-1}{\mu_a}\frac{(\mathbf{a}'.\mathbf{b})}{(\mathbf{a}.\mathbf{a}')}=\mu_a(\mathbf{a}.\mathbf{b})-\kappa^2(\mathbf{t}.\mathbf{b})$$

$$=\frac{r_{ag}r_{bg}}{r_{ag}}-\kappa^2 r_{tb}\sigma_t$$

$$=r_{bg}-\sqrt{1-\kappa^2}\,r_{tb}=0,$$

as follows at once from the expression for g in terms of t and i.

Further if we substitute

$$a'_s=\frac{\mu_a(\mathbf{a}.\mathbf{a}')}{\mu_a{}^2-1}\{\mu_a a_s-\kappa^2 t_s\},$$

in (9) we obtain

$$\Delta_r=\left\{\frac{\mu_a(\mathbf{a}.\mathbf{a}')}{\mu_a{}^2-1}\right\}^{k+1}\Delta'_r,$$

where

$$\Delta'_r=\begin{vmatrix}(\mu_a a_r-\kappa^2 t_r) & (\mu_a a_{r+1}-\kappa^2 t_{r+1}) & \cdots & \mu_a a_{r+k}-\kappa^2 t_{r+k}\\ a_r & a_{r+1} & \cdots & a_{r+k}\\ b_r & b_{r+1} & \cdots & b_{r+k}\\ \cdots & \cdots & \cdots & \cdots\\ p_r & p_{r+1} & \cdots & p_{r+k}\end{vmatrix}$$

If we multiply the 2nd, 3rd and 4th ... rows by

$$\kappa^2\frac{\mu_a}{\mu_a{}^2-1},\quad \kappa^2\frac{\mu_b}{\mu_b{}^2-1},\quad \kappa^3\frac{\mu_c}{\mu_c{}^2-1},\quad \ldots,$$

respectively, and add we obtain

$$\Delta'_r=\begin{vmatrix}\mu_a a_r & \mu_a a_{r+1} & \cdots & \mu_a a_{r+k}\\ a_r & a_{r+1} & \cdots & a_{r+k}\\ b_r & b_{r+1} & \cdots & b_{r+k}\\ \cdots & \cdots & \cdots & \cdots\\ \cdots & \cdots & \cdots & \cdots\\ \cdots & \cdots & \cdots & \cdots\\ p_r & p_{r+1} & \cdots & p_{r+k}\end{vmatrix}=0.$$

Thus the equations (9) as well as the equations (8) are satisfied, and (7) gives us the values of $a'_1, a'_2, \ldots, a'_n$ which will satisfy equations (8) and (9).

Similarly the values of the b's, c's, etc. which satisfy equations corresponding to (8) and (9) will be given by (7).

Thus (7) gives the true values of $\mathbf{a}'$, $\mathbf{b}'$, $\mathbf{c}'$, etc. and hence

$$\mathbf{r}_0 = \kappa^2 \mathbf{t},$$

and so

$$\mathbf{v} = \kappa \mathbf{i}.$$

Thus Wilson and Piaggio are presenting exactly the same state of affairs in different ways. It follows that g is not determinate but that its indeterminacy can be made as small as we please by taking a sufficient number of tests. For the standard deviation of i is unity, and κ^{-2} can be made as small as we please by taking enough tests.

Wilson, however, contends that this uniqueness is relative to the set-up, since, if we constructed artificial tests by taking linear combinations of the marks in the original tests in such a manner as to preserve the hierarchical conditions, the new g would not necessarily be the same as the old.

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FURTHER NOTES ON THE SINGLE GENERAL FACTOR IN MENTAL MEASUREMENTS.

BY J. C. MAXWELL GARNETT.

I.

EDWIN B. WILSON, in his review (*Science*, March 2, 1928) of Professor Spearman's book on *The Abilities of Man*, observes that: "It seems to be an undeniable statistical fact that batteries of intelligence tests as given and as scored tend to be what has been termed hierarchical in that they tend to satisfy the tetrad relations"

$$\frac{r_{ac}}{r_{ad}} = \frac{r_{bc}}{r_{cd}} \quad (a, b, c, d = 1, 2, \ldots, n) \qquad \ldots\ldots(1),$$

where r_{ab} is the coefficient of correlation between the measures q_a and q_b of any two out of a set of n (>3) mental measurements, $q_1, q_2, \ldots, q_n$, of the same individual. We assume all the q's are measured in such units as make their standard deviations unity.

But the statistical fact which had been demonstrated by the "huge array of evidence" collected by Prof. Spearman up to ten years ago was somewhat different. It was that the correlation between every pair of columns in the table

$$\begin{array}{ccccc} - & r_{12} & r_{13} & \cdots & r_{1n} \\ r_{12} & - & r_{23} & \cdots & r_{2n} \\ r_{13} & r_{23} & - & \cdots & r_{3n} \\ \multicolumn{5}{c}{\cdots\cdots\cdots\cdots\cdots\cdots} \\ r_{1n} & r_{2n} & r_{3n} & \cdots & - \end{array} \qquad \ldots\ldots(2),$$

tended to be ± 1.

The consequences of the second fact (correlation between columns) are not the same as those of the first (tetrad relations) which is a particular case of the second. The "correlation between columns" conditions establish the same number $\frac{1}{2}n(n-3)$ of independent relations between the correlations of the q's as the tetrad equations have been shown (Garnett, *Brit. J. Psychol.* March, 1920, p. 245) to yield. When the tetrad equations (1) are satisfied, it follows that

$$q_s = r_{sg} . g + \sqrt{1 - r_{sg}^2} . x_s \; (s = 1, 2, \ldots, n) \qquad \ldots\ldots(3),$$

where $g, x_1, \dots, x_n$ are $n+1$ independent variables having unit standard deviations and where

$$r_{sg}^2 = \frac{r_{as} r_{bs}}{r_{ab}} = \frac{r_{cs} r_{ds}}{r_{cd}} = \dots \qquad \text{......(4).}$$

But when the "correlation between columns" conditions for (2) are satisfied, the consequence is that

$$\frac{r_{ab} r_{cd} - r_{ad} r_{bc}}{r_{ad} - r_{cd} - r_{ab} + r_{bc}} = \text{constant} = C, \text{ say } (a, b, c, d = 1, 2, \dots, n) \text{......(5),}$$

as was proved in 1919 (Garnett, *Proc. R.S.* A, 1919, XCVI, 105). When $C = 0$ these reduce to the tetrad equations (1). Moreover C is positive whenever the r's in table (2) (arranged as usual so that the largest r's, irrespective of sign, are in the top left-hand corner and the smallest in the bottom right-hand corner) diminish in absolute magnitude more rapidly than in a perfect hierarchy[1]: more rapidly, that is, than when $C = 0$ and the tetrad equations hold. Or, what amounts to the same thing, C is positive when the qualities tested are sufficiently dissimilar: less similar, in fact, than would produce a perfect hierarchy[2]. When, in the sequel, we speak of the q's being sufficiently dissimilar, we shall mean that they are not more alike than is necessary for a perfect hierarchy and that therefore $C > 0$.

In that case (when $C > 0$), we may put $C = K^2$, and it has been proved (Garnett, *loc. cit.* p. 105) that

$$(q_s - Ky)(1 + K^2)^{-\frac{1}{2}} = R_{sg} . g + \sqrt{1 - R_{sg}^2} . x_s \; (s = 1, 2, \dots, n) \qquad \text{...(6).}$$

In these equations $g, x_1, \dots, x_n$ are $n+1$ independent variables having unit standard deviation and y is a variable independent of all the q's but obviously in the same $n+1$-dimensional space as the x's and g. Moreover, y has the same unit standard deviation as the other variables. As already stated,

$$K^2 = \frac{r_{ab} r_{cd} - r_{ad} r_{bc}}{r_{ad} - r_{cd} - r_{ab} + r_{bc}} \; (a, b, c, d = 1, 2, \dots, n) \qquad \text{......(5').}$$

Finally

$$R_{sg}^2 = \frac{(r_{as} + K^2)(r_{bs} + K^2)}{r_{ab} + K^2} = \frac{(r_{cs} + K^2)(r_{ds} + K^2)}{r_{cd} + K^2} = \dots \qquad \text{......(7).}$$

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[1] The statement, in the original proof of equation (5) (*loc. cit.* p. 103) that C can always be made positive by suitably choosing the signs of the q's, was erroneous.

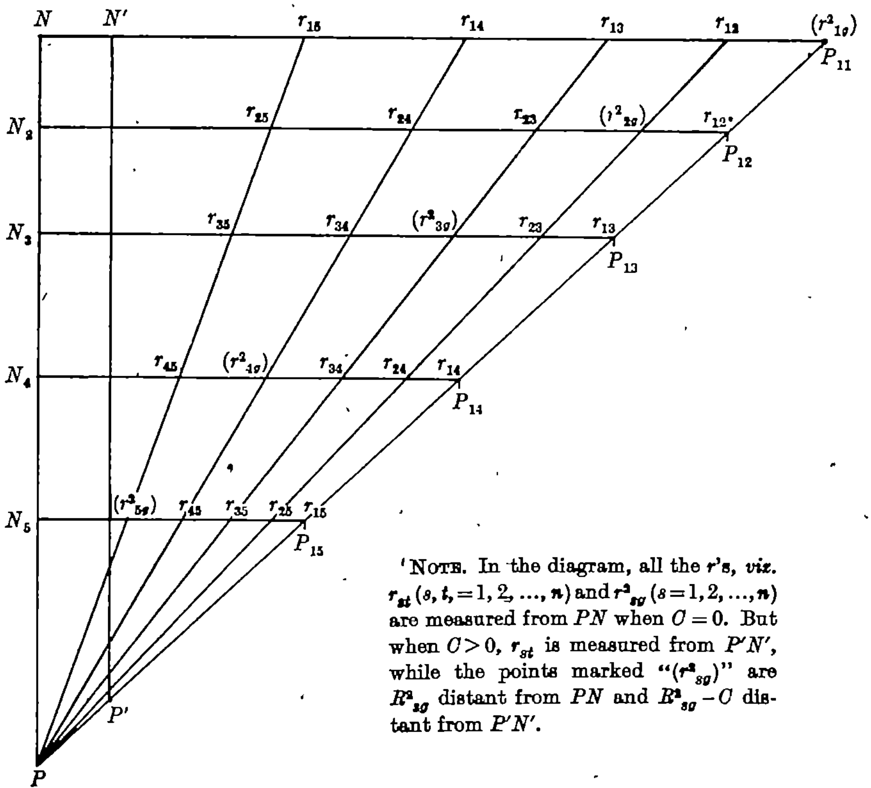
[2] Every correlation coefficient, when the "correlation between columns" conditions are satisfied, will be less by *the same amount* than the corresponding coefficient of a set which satisfy the tetrad equations.

In order to see that the "correlation between columns" conditions give $\frac{1}{2}n\,\overline{n-3}$ independent relations between the $\frac{1}{2}n\,\overline{n-1}$ r's of table (2), it is only necessary to observe that equations (7) are $\frac{1}{2}n(n-1)$ in number. We may solve n of them for $R_{1g}, R_{2g}, \ldots, R_{ng}$ and we are left with

$$\tfrac{1}{2}n(n-1) - n = \tfrac{1}{2}n(n-3),$$

independent relations between the r's and K^2; and K^2 is determined by equation (5′). Thus the "correlation between columns" conditions, like the tetrad equations, are equivalent to $\frac{1}{2}n(n-3)$ independent relations between the $\frac{1}{2}n(n-1)$ r's.

The following diagram represents geometrically the most general case in which the "correlation between columns" conditions are satisfied between the r's in (2). The magnitudes of $r_{12}, r_{13}, \ldots, r_{1n}$ are first measured from left to right along NN'. Then other points also marked $r_{12}, r_{13}, \ldots, r_{1n}$



NOTE. In the diagram, all the r's, *viz.* r_{st} $(s, t, = 1, 2, \ldots, n)$ and r^2_{sg} $(s = 1, 2, \ldots, n)$ are measured from PN when $C = 0$. But when $C > 0$, r_{st} is measured from $P'N'$, while the points marked "(r^2_{sg})" are R^2_{sg} distant from PN and $R^2_{sg} - C$ distant from $P'N'$.

are plotted with the same abscissae but at distances below NN' such that the difference of these distances are equal to (or proportional to[1]) the differences of the abscissae. Then these points lie on a line PP'. Call these points $P_{12}, P_{13}, \ldots, P_{1n}$. Through these points draw lines parallel to NN'.

Then r_{23} may have any position (to the left of r_{13}) on $P_{12}N_2$.

Join r_{13} on NN' to r_{23} on $P_{12}N_2$ and let it meet PP' in P.

The $\frac{1}{2}n\ (n-3)$ independent relations between the r's suffice to determine the positions of the $\frac{1}{2}n\ (n-3)$ remaining r's, namely

$$r_{24}, r_{25}, \ldots, r_{2n};\quad r_{34}, r_{35}, \ldots, r_{3n};\quad r_{45}, \ldots, r_{4n};\quad \ldots;\quad r_{n-1,n}.$$

When the axis $P'N'$, the distances from which measure the magnitude of the r's, lies to the right of PN, then C is positive. When $P'N'$ coincides with PN, $C=0$ and the tetrad equations are satisfied: the r's form a hierarchy. When $P'N'$ lies to the left of PN, C is negative and K is imaginary.

Moreover (using for the moment the symbols $r_{12}, r_{13}, \ldots$, to denote the points so marked along NN') we have

$$R_{1g}^2 = \frac{r_{12}N\,.\,r_{13}N}{r_{23}N} = \frac{PN}{PN_2}\,.\,r_{12}N = \frac{PN}{PN_2}\,.\,P_{12}N_2 = P_{11}N,$$

where P_{11} marks the intersection of PP' and NN'. Thus $P_{11}N' = R_{1g}^2 - K^2$, etc. If the tetrad equations are satisfied, so that $K=0$, $R_{sg} = r_{sg}$, then $P_{11}N = r_{1g}^2$.

II.

E. B. Wilson ("On Hierarchical Correlation Systems," *Proc. Nat. Acad. Sci.* March, 1928) and H. T. H. Piaggio (*Nature*, January 10, 1931) have shown that, when the tetrad relations are satisfied, g possesses two independent components, of which the first is determinate in quality and quantity and lies in the n-dimensional space of the q's, while the second is indeterminate in magnitude and, being independent of the q's, lies at right angles to their n-dimensional space, but of course in the same $n+1$-dimensional space as the x's and g. It may be shown that the same is true when the correlation between columns conditions are satisfied. In this case, the second component of g is a determinate multiple of y in equations (6).

These results may be demonstrated as follows.

First suppose that the tetrad relations (1) are satisfied.

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[1] (Added 13. vi. 1931.) Thus N_2P_{12} can be at any distance below NN'; but, once this distance is decided, the intersection of $r_{13}r_{23}$ and $r_{14}r_{24}$ fixes P_1 and therefore also the line PP_{12}.

If G is the most probable value of g for the given q's then, of all the individuals whose q's are measured, that one whose $g = G$ is as nearly average as his measured q's permit. Hence, in the $\overline{n+1}$-dimensional space of equations (3),

$$g^2 + x_1^2 + x_2^2 + \ldots + x_n^2 = \text{a minimum},$$

or, from (3),
$$g^2 + \Sigma \frac{(q_s - r_{sg} \cdot g)^2}{1 - r_{sg}^2} = \text{a minimum}.$$

Differentiating with respect to g, this gives the corresponding value of g, namely

$$G = \frac{\Sigma \frac{q_s r_{sg}}{1 - r_{sg}^2}}{1 + \Sigma \frac{r_{sg}^2}{1 - r_{sg}^2}} \qquad \text{......(8).}$$

(A result given in my letter to Prof. Spearman dated September 25, 1930.)

G thus lies in the n-dimensional space of the q's. Since g lies in the $\overline{n+1}$-dimensional space of the x's and g, we may write

$$g = G + ky \qquad \text{......(9),}$$

where y is independent of the q's, being measured at right angles to their n-dimensional space in the $\overline{n+1}$-dimensional space of the x's and g's and where k is to be determined so that the standard deviation of y is unity. Thus from equations (8) and (9) we get

$$y = \frac{1}{k}(g - G)$$

$$= g/k \Big/ \left(1 + \Sigma \frac{r_{sg}^2}{1 - r_{sg}^2}\right) - 1/k \, \Sigma \frac{x_s r_{sg}}{\sqrt{1 - r_{sg}^2}} \Big/ \left(1 + \Sigma \frac{r_{sg}^2}{1 - r_{sg}^2}\right).$$

In this equation, the sum of the squares of coefficients of the $\overline{n+1}$ independent variables must be unity, if y is to have the same unit standard deviation. Consequently

$$k^{-2} = 1 + \Sigma \frac{r_{sg}^2}{1 - r_{sg}^2} \qquad \text{......(10),}$$

as stated by Piaggio[1]. Equations (3) now give x_s $(s = 1, 2, \ldots, n)$ in terms of the measured q's and y.

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[1] (Note added June 20th, 1931.) Dr Irwin has pointed out to me that, according to this equation, k tends towards zero as the number of tests is increased. This tendency is all the greater when the qualities tested correlate highly with g.

III.

In order to effect the corresponding analysis of g when the "correlation between columns" conditions in (2) are satisfied, and the q's are sufficiently dissimilar, so that K is real in equations (6), we observe that, according to equations (6), the most probable value, G, of g for the measured q's will be got by putting $y = 0$ and making

$$g^2 + x_1^2 + x_2^2 + \dots + x_n^2 = \text{a minimum}.$$

This gives, by the same reasoning as before,

$$G = \frac{1}{\sqrt{1+K^2}} \cdot \frac{\Sigma \dfrac{q_s R_{sg}}{1 - R_{sg}^2}}{1 + \Sigma \dfrac{R_{sg}^2}{1 - R_{sg}^2}}.$$

As before, we may put

$$g = G + ky \qquad \text{......(9')},$$

where y is measured in the direction which is perpendicular to the n-dimensional space of the q's in the $\overline{n+1}$-dimensional space of the x's and g so that, when k has been chosen to give y a unit standard deviation, y is the same as in equations (6).

In order to determine k, we proceed as before and obtain a quadratic equation for k in terms of the R's and K and therefore in terms of the r's, from equations (5') and (7). Thus

$$k = -\frac{K}{\sqrt{1+K^2}} \cdot \frac{\Sigma \dfrac{R_{sg}}{1 - R_{sg}^2}}{1 + \Sigma \dfrac{R_{sg}^2}{1 - R_{sg}^2}} \pm \left\{1 + \Sigma \frac{R_{sg}^2}{1 - R_{sg}^2}\right\}^{-\frac{1}{2}} \qquad \text{...(10')}.$$

In this expression for k, the second term, but not the first (which is due to the "y" term in equation (6)), approaches zero as the number of tests is increased. Equations (6) now give x_s $(s = 1, 2, \dots, n)$ in terms of the measured q's and y.

IV.

It may be observed that, when all the tests correlate highly with each other so that all the r's are nearly unity,

(a) if the tetrad relations hold

$$q_1 = q_2 = \dots = q_n = g;$$

(b) if the "correlation between columns" conditions hold, K becomes imaginary and the equations (6) are without significance.

V.

When all the qualities tested are very dissimilar so that all the r's are nearly zero,

(a) if the tetrad relations hold, equations (3) become

$$q_s = x_s \; (s = 1, 2, \ldots, n) \qquad \ldots\ldots(11);$$

(b) if the "correlation between columns" conditions hold, equations (5′) and (7) give $K = 0$ and $R = 0$, so that equations (6) give again

$$q_s = x_s \; (s = 1, 2, \ldots, n).$$

In this case $k = 1$ and $g = y$, so that g is wholly indeterminate.

VI.

The result expressed in equations (11), obvious though it is, appears to have been overlooked in the last two sentences on page 105 of the present writer's first paper on this subject (*Proc. R.S.* A, 1919, XCVI). It is not true, as was there stated, that, when the "correlation between columns" conditions hold between the correlations of a large number of *sufficiently dissimilar* mental tests, then the tetrad relations also hold: except, of course, when the tests are so very dissimilar that, as above, $K = 0$ and $q_s = x_s$ $(s = 1, 2, \ldots, n)$ as in equations (11).

VII.

A paper by the present writer published in this *Journal*, March, 1920, quoted the above erroneous statement without correcting the mistake (p. 254). In the same paper (p. 253) it was shown that g, the single general factor which, along with a specific factor enters, according to equations (3), into each of n q's whose correlations satisfy the tetrad relations, is uniquely determined *in quality*, *i.e.* in direction "in a space of $n + 1$ dimensions." The words here italicized were omitted from the verbal expression of the conclusion (p. 253), although the mathematical demonstration related only to the quality (direction), and not to the quantity (magnitude) of g. Moreover, the words in inverted commas were not repeated in the summary of conclusions (p. 257) which incorrectly stated that "the single general factor...is *unique.*" It is unique *in quality in a space restricted to* $n + 1$ *dimensions*. But, as Wilson points out (*Proc. Nat. Acad. Sci.* March, 1928, p. 289), there is more room. The quality y in equations (9) may be measured in *any* direction which is perpendicular to the n-dimensional space of the q's. And the direction of g depends upon the direction of y and the magnitude of g upon the magnitude of y.

VIII.

The correlations between mental measurements tend to satisfy the "correlation between columns" condition (2). Unless they further tend to satisfy the tetrad relations (1), the essential fact about a set of sufficiently dissimilar mental measurements is expressed by equations (6) above, rather than by equations (3). That fact is that the difference be-between each measurement (*e.g.*, q_s) and a determined multiple (K) of another variable (y) which is independent of all the measured q's, is made up of a single general factor (g) and a specific factor (x_s) which is independent of every other measurement (*i.e.* of every q except q_s) and independent of g but not of y. Moreover, according to equation (9′), g is made up of a component (G) which is determined in magnitude and direction—it lies in the n-space of the q's—and a determined multiple (k) of the variable y.

It may be observed parenthetically that, if the tetrad relations hold between the correlations of the q's, the essential fact about the set of measurements is much the same, although now it is expressed by equations (3) and (9) instead of by equations (6) and (9). The chief differences are, firstly, that y has a different coefficient in the n equations which govern, although they do not determine, the relations between the measured q's, the specific x's and y; and, secondly, that the coefficient of y tends to vanish as the number of tests is increased.

IX.

What is the psychological interpretation of the essential fact in either case? That is a question for psychologists; and the most interesting part of it is perhaps the nature of y. For y measures a quality of the individual tested (in such units that the standard deviation is unity). And, since y is independent of all the q's, there seems no reason why y should not be *the same* for the same individual *in every set of mental tests.*

If, for example, y measured the individual's "power of concentration"—perhaps how much "mental energy" he renders available by a unit "effort of Will"—y might be independent of his performances $q_1, \ldots, q_n$ in any particular set of tests. Then g might measure how much "mental energy" he manifests in the set of tests, so that g would depend on y and would be common to all the tests of the set. Finally, x_s might measure, as Prof. Spearman has suggested, the individual's specific ability in the sth test: how skilful he is. The amount of his skill (x_s) might well depend on his power to learn, his power to concentrate, and thus on y, while remaining independent of the effort he makes in the particular set of tests in question. Thus the relations between q_s, y, g and x_s set forth in equations (6)—or in equations (3)—and in equation (9) are such as would obtain if

q_s measures the individual's performance in the sth test,
x_s „ how skilful he is at that test,
g „ how much he tries throughout the set of tests,
y „ how much good his trying does.

But this is, of course, only a rough guess and probably a wrong one. It may perhaps serve as a hypothesis to be tested and corrected by experiment.

(*Manuscript received* 16 *October*, 1931.)

PUBLICATIONS RECENTLY RECEIVED.

Change of Interests with Age. By E. K. Strong, Jr. Stanford, California: Stanford University Press. 1931. (English agents, Oxford University Press.) Pp. xix + 235. 24*s*.

Prof. Strong points out that we know far more about the psychology of children and of young adults than of older persons. By recasting some of the extensive data that he has collected in the field of vocational interests, he is able to offer in this book certain significant conclusions as to the likes and dislikes of men ranging in age from 20 to 60. Naturally the author could not trace the changes in the interests or aversions of particular individuals over 40 years, as the title of the book might suggest. His data are more restricted in scope, being based mainly on the averages or percentages of some 2000 men, who checked the various items (according to their like or dislike) in his Vocational Interest Blank. By this method it is found that older subjects have as broad a range of interests as younger men; but their preferences run less to physical exploits, etc., more to solitary and cultural activities. The greater proportion of these changes seem to occur near the beginning of the period of life that was studied. It is also shown that the error in the prediction of a subject's mature vocational interests is fairly small when he is above the age of 20, though probably much greater at younger ages.

Detailed tables of the results for each interest item make amusing reading, and many of the statistical techniques are noteworthy.

Institute for Child Guidance Studies. Selected Reprints. Edited by Lawson G. Lowry. New York. 1931. The Commonwealth Fund. Pp. viii + 290. $1.50.

The papers reprinted in this volume have been collected from various scientific and professional journals in America. They are roughly classified as Social, Medical, Psychological, and pertaining to Child Training. They are all the work of members of the staff of the New York Institute for Child Guidance, and, while they are of unequal degrees of merit, the volume as a whole contains much to interest and help the social worker and the psychologist whose concern it is to study and direct young children. For the experimentally-minded investigator much the most interesting paper is the one by Samuel J. Beck on *The Rorschach Test and Personality Diagnosis: The Feebleminded.* The test and its method of evaluation are adequately described, and some interesting original applications are discussed.

Les Examens d'Aptitude Professionnelle: Théorie et Pratique. Par Franziska Baumgarten. Traduit de l'Allemand par Marcel Thiers. Paris: Dunod. 1931. Pp. xxix + 654.

No doubt many English readers will be glad to have this French translation of Dr Baumgarten's well-known and comprehensive treatise. The book contains an amazing amount of detail, and in spite of the rapid development of the subject it must for some time to come prove a very valuable work of reference for all industrial psychologists.

The First Two Years: A study of twenty-five babies. University of Minnesota Institute of Child Welfare. Monograph Series, No. VI. By MARY M. SHIRLEY. Vol. I. Postural and Locomotor Development. Minneapolis: University of Minnesota Press. 1931. Pp. xv + 227. $2.00.

It is curious that until recently direct observational and experimental records of the very early development of the human infant were very few in number. This lack is now being rapidly made up, and Dr Shirley's study is undoubtedly one of the best of the monographs now available. She explains very clearly and concisely her methods and their results, and discusses briefly the theoretical implications of the results. She claims that motor development in young children is in no sense a haphazard trial and error matter, but follows a series of well-discriminated patterns, and that each of these, in its active operation, develops through a number of definite stages. The monograph is pleasantly illustrated, and includes a useful bibliography. As a record of original and valuable observations this book well deserves careful study.

The Psychology of Foreign Language Study. By H. R. HUSE. University of North Carolina Press. 1931. (London: Humphrey Milford.) Pp. viii + 231. 13*s*. 6*d*. net.

In the main this consists of a review of experimental work on the learning of foreign languages. First the author reviews work on methods of teaching, and on some problems of learning and recall. Then he states some current doctrines and practices in relation to his problem. He works out a "suggested basis for a science of language teaching," and indicates problems that should repay experimental study. An appendix contains an account of an experiment in learning word-pairs. It is maintained that "if the intention is to memorize units of expression as units for recognition either singly or in varying combinations, the method of presenting them in a fixed unfamiliar context is extremely wasteful."

So far as it goes the book is very clear and should be useful. It makes no pretence to cover more than a small part of the ground, but contains a useful bibliography. The price seems excessive.

L'Âme des Bêtes: Quelques pages d'histoire. Par L. VERLAINE. Paris: Félix Alcan. 1931. Pp. 202. 25 fr.

This is a rapid descriptive sketch of views concerning the mental life of animals from Greek times to the present day, tending to show a constant stream of development towards genuine biological methods of investigation and interpretation. We are coming to see, concludes the author, that "there is not physiology on the one side and psychology on the other, but biology, having the same conditions and laws throughout, but with multiple manifestations."

Psychologie. Par P. GUILLAUME et G.-H. LUQUET. Paris: Félix Alcan. 1931. Pp. 388. 30 fr.

This is intended as an elementary text-book of psychology. It is clearly written and provided with numerous illustrations. It deals with all the usual psychological problems, though there is comparatively little about special sense reactions. Instinct, tendencies, affect and emotion are dealt with first. Then follow sections on perception, memory, imagination, the laws of association, attention, volition and personality. Thought and language receive special treatment and this leads up to a discussion of belief and reasoning. There is a chapter on intelligence and the book closes with a brief study of the limits of consciousness.

The Jungle of the Mind. By E. J. SWIFT. New York and London: Charles Scribner's Sons. Pp. 340.

In this entertaining book Dr Swift tells a huge number of odd stories about the various ways in which human beliefs and opinions are formed, and in which human reasoning proceeds. In 331 pages he presents the mind as a jungle and in the remaining 4 pages of the text he indicates how he thinks we may find "the trail out of the jungle." The directions are less explicit than the description. The book is wholly popular in tone, but a teacher will find in it an abundance of useful illustrations, and the "general reader" will find much to set him thinking.

Aristotle's Psychology of Conduct. By A. K. GRIFFIN. London: Williams and Norgate. 1931. Pp. 186. 15*s.* 6*d.* net.

This book gathers together the views about psychological problems found in various works by Aristotle and presents them as a consistent whole. The exposition is clear and scholarly. It is descriptive and not critical.

The Problem of Genius. By W. LANGE-EICHBAUM, trans. by E. and C. PAUL. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. xix + 187. 8*s.* 6*d.* net.

This is a translation of the book entitled *Das Genie-Problem, eine Einführung*, which has already been briefly noticed in this *Journal*. As is already pretty well known, the thesis is that genius is mainly if not wholly a matter of what a representative group chooses to treat as of outstanding interest and importance. This thesis is argued vigorously and with many illustrations. Perhaps it does not go into the heart of the matter, but it gives the author a chance to say many interesting things about genius and insanity, genius and talent, and genius and civilization.

The Neural Energy Constant: A Study of the Bases of Consciousness. By JOHN BOSTOCK. London: Geo. Allen and Unwin, Ltd. Pp. xv + 178. 6*s.* net.

Consciousness has developed gradually. At the beginning it is concerned merely with providing data for adjustment. It is then a matter of pure, sensorial awareness. Biologically the organism must avoid extremes of stimulation and of "simple physical dangers" and hence grew up an "emotion centre" which made coarse adjustment possible. On the top of this developed a third system of discriminatory responses resulting in a possibility of fine adjustment. "Complete consciousness" depends upon a functional interrelation of all three systems. As the neurone, which is the basic structure of all nervous activity, is very readily fatiguable, there must be: (*a*) insulation of neurones, (*b*) a minimum number of neurones in operation at each moment of consciousness, and (*c*) a neural energy constant. The implications in regard to the general theoretical basis of psychology are worked out by a critical examination of the notion of psychological levels; of sleep and dreams; of instinct and of mental disease.

Expressed in this summary fashion, the whole essay looks to be excessively speculative. In fact it is based upon a large amount of direct observation and experiment, and it is full of interest. Naturally it presents a great many points that call for criticism, but the author presents his views in a consistently clear and judicious manner, and has produced a study that is very well worth reading.

The Nature and Treatment of Stammering. By E. J. BOOME and M. A. RICHARDSON. London: Methuen and Co. 1931. Pp. 135. 5*s.* 6*d.* net.

This book is based upon first hand and fairly extensive experience with stammering children. With great lucidity and numerous case studies it discusses the causation, symptoms and treatment of stammering, insisting throughout that it is not a speech defect but a nervous disorder. The volume will be found genuinely helpful by all investigators who are concerned with its subject.

The Criminal, the Judge and the Public. By FRANZ ALEXANDER and HUGO STAUB, trans. by GREGORY TILBOORG. London: Geo, Allen and Unwin, Ltd. 1931. Pp. xx + 238. 10*s.* net.

Part I deals with The Problem of Crime in the Light of Psycho-analytical Theory, and Part II with Some Criminal Cases in the Light of Psycho-analysis. The treatment consists of a considerable number of brief essays or lectures rather than of a sustained and progressive argument. Hence there is a good deal of repetition. The general views are wholly in line with theories accepted within the psycho-analytic school. A good many criminals—but not all—are merely gratifying an emotional urge which has become attached to what the community considers to be the 'wrong' type of object. It is equally true, the authors think, that most current methods of punishment are merely an emotional gratification of social affects. The translation is often rather wooden.

The Marquis de Sade. By OTTO FLAKE. Trans. by EDWARD CRANKSHAW. London: Peter Davies. 1931. Pp. 230. 10*s.* 6*d.* net.

This translation from the German gives an account of the life and times of the Marquis de Sade, together with a summary of his main writings. It also contains a chapter dealing with Restif de la Bretonne. The book is written in the name of psychology, but it has comparatively little psychological value.

The Craving for Superiority. By RAYMOND DODGE and EUGEN KAHN. Yale University Press. 1931. (London: Humphrey Milford.) Pp. vii + 69. 7*s.* net.

"Probably," say the authors, "no person or community may be entirely free from the craving for superiority." In the course of eight brief essays they make a number of observations about varieties and expressions of this craving. The essays are interesting enough to read and might provide debating societies with a number of nice points. In a psychological sense, however, the book does not seem to amount to very much. The English price seems excessive.

Joan: A Story from Life. By EDITH E. READ MUNFORD. London: Longmans, Green and Co. 1931. Pp. 87. 2*s.* 6*d.* net.

This is an account of how a naughty little girl was turned into a good little girl by the kind ministrations of Nurse Hope. It is very pleasantly written and it contains more commonsense than many more pretentious psychological treatises. The book has a number of quite good but rather formal pencil sketches by Jeannie McConnell.

Desuggestion for the Attainment of Health, Happiness and Success. By E. TIETJENS, trans. by E. and C. PAUL. London: Geo. Allen and Unwin, Ltd. 1931. Pp. 593. 18*s.* net.

The author considers that all individuals nourish large numbers of fads, fancies, hypothetical and unreal "guilts" and "wickednesses." These have been implanted in him by powerful suggestion, and he cannot be healthy, happy, virtuous or successful until, by process of "desuggestion," he gets rid of them. This book discusses these fancies and their direful results at great length and with much vigour. The head and fount of all human fallacies is that of free will. Throughout the book the rather forbidding terminology of Semon is adopted. The argument could have been expressed equally well in about a quarter of its present length.

My Life in Two Worlds. By GLADYS O. LEONARD. London: Cassell and Co. 1931. Pp. ix + 300. 7*s.* 6*d.* net.

This is an autobiography written by a well-known medium, and contains a brief appreciative preface by Sir Oliver Lodge. On the whole, Mrs Leonard writes in a straightforward manner, without an undue amount of interpretation. Much of the work, especially that which relates some of her early childhood experiences, is of genuine interest, and should repay careful study on the part of any psychologist who has a leaning towards spiritualistic investigations.

Psychologia das Sensações. By ARAULD BRETAS. Rio de Janeiro: Paulo Pongetti Cia. 1930. Pp. 104.

This discussion deals mainly with general characteristics or properties of special sense responses, but contains also some study of the particular phenomena of the leading sensory processes.

Psychologia da Attenção. By Dr A. UBIRAJARA DA ROCHA. Rio de Janeiro: Engenho de Dentro. 1930. Pp. 94.

The topics dealt with are the "field of consciousness," the "current of consciousness," a brief genetic study of attention, and some consideration of the experimental psychology of attention. The whole is based upon the theory of affective discrimination put forward by Radecki.

Psychologia do Pensamento. By LUCILIA TAVARES. Rio de Janeiro: Engenho de Dentro. 1930. Pp. 130.

This study deals generally with the thinking process, and its immediate predecessors, in the way of perceiving, imaging and recall, in mental life. There is a section on experimental investigations.

Exame Psychologico da Criança. By HALINA RADECKA. Rio de Janeiro: Engenho de Dentro. 1930. Pp. 152.

Contributi del Laboratorio di Psicologia. Pubblicazioni della Università Cattolica del Sacro Cuore. Serie Quinta. Milano: Società Editrice "Vita e Pensiero." 1931. Pp. 572.

The contributions to this volume deal with: (1) Experimental Psychology, (2) Applied Psychology, (3) Industrial Psychology, and (4) Comparative Psychology. Dr A. Gemelli and Dr A. Galli are responsible for the bulk of the work. Especially valuable are the papers on perception in the first section. These have been very largely influenced by the *Gestalt* movement, but they show a wide knowledge of current experimental work, and are a clear account of well-devised experiments. Taken in sum, the volume bears impressive witness to the energy and very great capability of the Director of the Laboratory concerned, and it deserves wide welcome and careful study.

Zur Biologie der Ethik. Von OTTO KANT. Wien: Julius Springer. 1932. RM. 8.80.

This monograph represents an interesting incursion into ethics by a psychiatrist. He considers in considerable detail the various meanings of the feeling of guilt, of responsibility or of "ought," following its treatment in psychological literature, its character at different stages of development, and its appearance and nature among neurotic and psychotic patients. The concluding portion of this section deals with Tolstoy's ethical doctrines. The rest of the monograph is mainly concerned with conclusions which may be drawn from the facts and views set forth in the first part, particularly with respect to the problem of the freedom of the will. The whole essay is very well worth study. Whether or not the biological approach adopted has any ultimate significance for ethical problems, the facts and reflections presented have an importance that is too often overlooked.

Lehrbuch der Charakterkunde. Von A. KRONFELD. Berlin: Julius Springer. 1932. Pp. 451. RM. 24.

This is a very good comprehensive treatise on the modern methods of studying character. After a brief general introduction the author considers current psychologies of types. He next attacks the question of ethical characteristics in relation to personality study with some reference to ethico-religious systems. The third section deals with philosophical systems of character study. Here Ludwig Klages, perhaps inevitably, gets a good deal of attention. There are also, somewhat oddly in this section, brief accounts of the development of physiognomic and graphological methods of approach. A consideration of rationalistic philosophical speculation in relation to personality leads up to a more detailed account of biological and clinical methods of approach. Jaensch and Kretschmer both get attention here. The final section deals with genetic, sociological, and psycho-analytical studies, and the whole ends by a consideration of the views of so-called "Individual" psychology and its development. The writing is clear and competent and the book is a remarkably good general survey of its field.

Formal Logic. By F. C. S. SCHILLER. 2nd edit. London: Macmillan and Co. 1931. Pp. xxii + 423. 12*s.* 6*d.* net.

Comparatively few changes, and those of no radical character, have been introduced into the second edition of this well-known book. Dr Schiller is, as all the world knows, unusual among logicians in that he desires to give psychology a pretty extensive function in logic. It is perhaps a little unfortunate that he is so keen on criticism that he does not allow himself to develop the psychological implications of his views very constructively or fully. One gets a trifle tired of being told that because certain considerations are psychological they have mistakenly been ruled out of logic. It would be more interesting to know precisely what differences they would make if they were ruled in. Still the more logically-minded psychologist will no doubt continue to find much pleasure from the study of Dr Schiller's provocative book.

Impassioned Clay. By LLEWELYN POWYS. London: Longmans, Green and Co. 1931. Pp. 120. 6*s*. net.

In this essay Mr Powys attempts an imaginative reconstruction of human history together with many digressions about the beauty and significance of natural objects. The psychologist who desires a change from the pedestrian activities of his laboratory, and who can enjoy the delight of well-turned phrases and fitting words will gain much pleasure from this little book. Mr Powys is frankly Epicurean. He writes with controlled enthusiasm and an attractive, conscious sort of honesty.

An Essay Concerning the Understanding, Knowledge, Opinion and Assent. By JOHN LOCKE. Edited with an Introduction by BENJAMIN RAND. Cambridge, U.S.A.: Harvard University Press. (London: Humphrey Milford.) 1931. Pp. lx + 306. 15*s*. net.

This is a reprint of the original draft of Locke's famous essay, and has never before been given publicity. It is thus a book of considerable historical interest, and it has been beautifully produced.

Outlines of the History of Greek Philosophy. By EDUARD ZELLER. Translated from the 13th edition by L. R. PALMER. London: Kegan Paul, Trench, Trübner and Co. International Library of Psychology, etc. Pp. xv + 324. 15*s*. net.

Key to and Primer of Interlingua, or Latin without Inflections, meant to be used as an International auxiliary language amongst people of various Mother Tongues. London: Kegan Paul, Trench, Trübner and Co. 1931. Pp. 168. 4*s*. 6*d*. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

November 21, 1931.	"Frustration of Will Acts." By Rev. H. L. Philp, B.A. "Blood Distribution during Mental Activity." By Mr M. F. Lowe, B.Sc. Demonstration: "Perception of Tachistoscopically Exposed Symbols." By Mr A. G. Caws, B.Sc. Demonstration: "Effect of Affective Stimuli on Muscular Work." By Mr R. Westgate, B.Sc.
December 19, 1931.	"'Aura' Phenomena and the Negative After-Image." By Prof. D. F. Fraser-Harris, B.Sc., M.D., D.Sc., F.R.S.E. "A Universal Unit of Psycho-Physical Time." By Mr S. J. F. Philpott, M.C., D.Sc.

GENERAL MEETING ARRANGED BY THE AESTHETICS SECTION.

January 29, 1932.	"The Medium of Prose." By Mr Walter de la Mare.

SECTIONAL MEETINGS.

Medical.

November 28, 1931.	"Out-Patient Psychiatry." By Dr C. Stanford Read.
December 16, 1931.	Joint Meeting with Child Guidance Council. "Psychiatry in Russia." Speakers: Dr Edward Mapother; Miss Matthews; Dr Emanuel Miller; Dr Isobel Wilson.
January 27, 1932.	Chairman's Address: "The Myth of Progress."
February 24, 1932.	"The Mode of Operation of Psychotherapeutic Measures." By Dr Melitta Schmideberg.

Education.

December 1, 1931.	"Some Notes on the Incidence of Neurotic Difficulties in Young Children." By Dr Susan Isaacs.
January 4, 1932.	"The Difficult Child; Sociological Versus Individual Interpretations." By Dr Emanuel Miller.
February 1, 1932.	"The Child's Knowledge of Personality and Character." By Mr A. F. Watts, M.A.

Industrial.

November 25, 1931.	"Some Obstacles to the Determination of the Effect of Practice upon the Differences between Individual Performances." By Mr Julian Blackburn, B.Sc. (Econ.).

December 16, 1931.	"A Survey of Methods of Estimating or Testing Temperament and Personality with reference to their Present Value in Vocational Selection and Guidance." By Mr Philip E. Vernon, M.A.
January 13, 1932.	"Interest Measures and Their Validation." By Mr Douglas Fryer, Ph.D.
February, 17, 1932.	"Essential Data to be collected by the Industrial Psychologist—and the Methods." By Air Vice-Marshal Sir David Munro, K.C.B., C.I.E.

AESTHETICS.

December 4, 1931.	"Some Mental Factors in Stage Production." By Mr Russell Sedgwick.
February 25, 1932.	"A Psychological Analysis of Musical Composition." By Mr Philip Vernon.

SPECIAL LECTURES.

November 26, 1931.	"Suggested Parallels between Modern Speech and Clothing." By Prof. T. H. Pear.
January 26, 1932.	"The Medico-Psychological Aspect of Normality." By Dr Edward Glover.
February 26, 1932.	"Some Recent Advances in the Psychology of Childhood." By Dr Susan Isaacs.
March 15, 1932.	"Psychological Problems in an Industrial Setting." By Dr May Smith.

COMMITTEE FOR RESEARCH IN EDUCATION.

This Committee has completed its annual enquiry as to psychological research upon problems having an educational bearing. The following is a summary of the replies received.

RESEARCH PLANNED OR IN PROGRESS, DECEMBER, 1931.

BANGOR. The Training College.

An Investigation of Laws governing the Artistic and Constructive Work of Young Children.

BANGOR. University College.

Psychology of Mathematical Thinking.
Influence of Bilingualism upon Mental Processes.

BINGLEY. The Training College.

The Education of Physically Defective Children.

UNIVERSITY OF BIRMINGHAM.

A Statistical Enquiry as to the Correlation of School and University Successes.
Tests of Higher Mental Processes.
Studies in the Psychology of Early Childhood.
An Experimental Enquiry into the Suitability of the Otis Group Tests of Intelligence for use with Matriculated Students.
Problems of Colonial Education.
Preferences of Children and Adults in Poetry and Prose Literature.
Correlation between Performance in English Composition and Intelligence Tests.
Errors in English Composition as revealed in Essays written by Higher School Certificate Candidates and University Post-Graduate Students.
Sex Differences in Early Childhood in Spatial Observation and Visual Imagery.
The Development of Humour in Children.
Formal Training: an Historical and Critical Review with a re-examination in the light of Modern Psychology.
The Motives of English State Education, considered in relation to the problem of Educational Equality.
Psychology applied to Advertising.

BRIGHTON. Municipal Training College.

An Investigation into possible Remedial Treatment for Defects in Spelling and Arithmetic among Training College Students.

CAMBRIDGE. The Psychological Laboratory.

Personality and Temperament Studies.
"Whole" Method in the Learning of Skill.
An Analysis of Learning Curves.
The Influence of Background Configuration on Mental Work.
Interests, Perseveration and Temperament.

DARLINGTON. The Training College.

Study of Progress of Nursery School and Non-Nursery Children through Lower Stages of Elementary School.
Colour Preferences of Children between five and seven years and seven and eleven years respectively.

UNIVERSITY OF DURHAM. Armstrong College.

Employment Psychology.
The Development of Memory in its relation to the other Mental Functions.
The Effect of the Subconscious Association in Intelligence Testing.
An Investigation into the variation of Perseveration among School Children and its effect upon School Attainments.

EDINBURGH. St George's Training College.

Rhythmic Abilities and their relation to Education.
Preliminary Studies of Practical and Artistic Abilities.

UNIVERSITY OF EDINBURGH. George Combe Psychological Laboratory.

Methods of determining Mathematical Ability at an early stage in Post Primary Course.
Investigation of Temperament Tests.
Intelligence Tests for the Blind.
Group Tests of Colour Vision for use in Schools.
Motivation and Accessory Concomitants of Delinquency, with special regard to Adolescents.
Speed Factor in Intelligence.
Individual Differences in Suggestibility.
Psychological Study of Pictorial Art.
Psychological Study of Perception and Memory of the Relations and Development of Time Concepts in High School Pupils.

UNIVERSITY OF EDINBURGH. Department of Education.

Study of "Answer-Patterns" in Tests.
Inquiry into Absolute Scale Values for Test Items.
Comparison of Broadcast, Gramophone, Silent Reading and Oral Lessons.
Measurement of the Value of Oral Composition as a Teaching Method.
Binet Tests in two Indian Languages.
The Improvement of Methods and Materials in Native African Schools.
Mathematical Enquiries into Problems of Sampling.
Factors in Teaching, Learning and Testing Spelling.
Follow up Enquiries of Children of Known Intelligence.

HULL. Municipal Training College.

Individual and Comparative Child Studies.
Leisure Pursuits of Jewish and Non-Jewish Children of Different Ages.
Study of Creative Ability.
Psychological Study of value of Craft Work for School Children, Adolescents in Industry, and Adult Women respectively.

HULL. The Training College, Beverley Road.

Investigations to determine:

(1) How much of Children's Learning by Visual Imagery is really such, and how far it consists of translating Visual Images into other Images or Ideas, mainly by Association.
(2) How far the accepted relations of the Global and Partial Methods of Learning are absolute, or how far they depend upon Individual Variations in Retentive Power.
(3) How far definite appeals to "Original Thought"—e.g. in the Teaching of Literature—are superior to Formal Routine Work in their inducement to Independent Thinking on the part of the Children.

UNIVERSITY OF LEEDS.

The Prevalence of Eidetic Imagery and its Educational Significance.
The Reasoning of Children from six to eight years of age.
The Intelligence of School Children of some of the Castes in Travancore.
A Comparative Study of Physically Defective Children of Various Types.
Psychological Tests in relation to Achievement in Geography.
Some Mental Determinants of School Achievement.
Mechanical Aids to the Study of Geography.
The Conceptual Development of Adolescents.
The Speed of Associated Reproduction.

LEEDS. Yorkshire Training College of Housecraft.

Research into the Nature of Ability in Cookery.

LONDON. Child Guidance Clinic, Jewish Health Organization.

Relation of Position in Family to the Manifestations of Delinquency.
Nature of the Difficult Family.
Incidence of Inherited Mental Disabilities in the Neurotic and Difficult Child.
Relation of Mental Retardation to Delinquency.
Relative Importance of Social and Individual Interpretations of Behaviour Disorders in Children.

LONDON. Froebel Education Institute.

Detailed Studies of Behaviour of Individual Children in the Nursery Class.

LONDON. Institute of Medical Psychology.

Tests to demonstrate the Real Lines of Interest of Children.
Differentiation of Specific Types of Mind.

LONDON. THE LONDON CHILD GUIDANCE CLINIC.

The Bearing of Emotional Problems on Educational Retardation.
The Validity of the Intelligence Quotient in cases of Emotional Disturbance.
The Nature and Importance of Certain Specific Factors in cases of Reading Difficulty.
Perseveration as a Method of Diagnosis of Personality Types.

LONDON. NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

Vocational Guidance: Research in Progress in Borstal, Birmingham, Fife, Private Cases.
Occupational Analysis: (*a*) Secretarial Work; (*b*) Nursing; (*c*) Advertising; (*d*) Hotel Management.
Nature and Measurement of Mental Abilities involved in Factory Assembly Operations.
Rhythm in Motor Activity.
Tests for Motor Drivers.
Colour Discrimination.

UNIVERSITY OF LONDON. BEDFORD COLLEGE.

Emotional Development of Young Children.
Eyedness and Handedness (continuation).
Children's Thinking (continuation).
Part Played by Imagery in Learning (continuation).

UNIVERSITY OF LONDON. GOLDSMITH COLLEGE.

An Investigation into Spelling Ability and Disability of Training College Students.

UNIVERSITY OF LONDON. KING'S COLLEGE.

The Perception of Tachistoscopically exposed Words, Letters and Symbols.
Frustration of Will-acts and Conation.
Perseverance and Perseveration.
The Influence of Affection and Will on Ergographic Performance.
The Psychology of Evidence. Comparison of Testimony in Normal and Delinquent Youths.
Blood Distribution in Conative and Other States.
The Influence of Volition upon Thinking.
Friendship-Love in Adolescence.
Mental Factors in Aesthetic Creation and Appreciation.
The Automatization of Voluntary Movement.

UNIVERSITY OF LONDON. LONDON DAY TRAINING COLLEGE.

Scientific Interests of Girls 12–13 years old.
Science and Observation.
Factor of Oscillation.
The Measurement of Teaching Ability.
Practice, Fatigue and Oscillation.
The Transfer of Effects of Science Training.

Group Factor in Reasoning.
Extension of Studies in Educational Psychology for Teaching Purposes.
The Teaching of Science to the Chinese.
Cultural Value of Science for "Arts" Students.
Growth of Aesthetic Appreciation.
The Foundations of Elementary Geometry—an Experiment in Scientific Method.
Method of Teaching Biology.
Methods of Teaching English.
The Education of the Backward Child.

UNIVERSITY OF LONDON. University College.

Fluctuations in Human Output.
Comparative Value of Certain Verbal and Perceptual Tests and their relation to Tests of Practical Ability.
An Experimental Study of Influences entering into Certain Character Estimates, with special reference to possible Common-Factor Content.
Introspective Study of Difficulties in Mental Work.
An Experimental Investigation, in terms of *p*-Factor and *w*-Factor, of Difficult Children.
An Experimental Contribution to the Function of Imagery.
The Quality-Quantity Function of Intelligence.
Time Estimation.
Individual Differences in Reproduction of Completed and Uncompleted Test-Items.
The Diagnostic Value of *p*-Factor amongst School Children.
Factor Studies of the Pre-School Child.

The department's research assistants are engaged on a third year's work upon studies in experimental psychiatry. The nature of these researches can be gathered from the following titles of papers that are either published or in the press:

Studies in Experimental Psychiatry.
No. I. A Case of General Inertia. (*J. Mental Science*, Oct. 1931.)
No. II. Some Contact of *p*-Factor with Psychiatry.
No. III. *p*-Score and Inhibition for High-*p* Praecoxes.
No. IV. Some Contact of *g*-Factor with Psychiatry.
No. V. Certain Orectic Characteristics in Psychiatry.

Other studies by the research assistants are:

Transfer of Training in acquiring the Grammar of a Foreign Language.
Measurement of *g*-Deterioration in Psychiatry.
A Study of Fluency in Psychiatric Subjects.
Experimental Investigation into the *p*-Characteristics of Melancholia, Mania and Praecox Conditions.
Measurement of "Oscillation" for Psychiatrical Cases.
A Study of *p*-Factor in terms of Pavlov's Theories of Inhibition, for Normal and Abnormal Subjects.
The *g*-Variation for 400 Girls, after an interval of two years, in terms of the Tetrad Criterion.
A Study of the Distribution of Tetrad Differences for a large number of Intercorrelated Sub-tests of '*g*.'

UNIVERSITY OF MANCHESTER.

Range of Homogeneity of Ability in Normal and Defective Children.
Experimental Tests of Colour Discrimination.
Nature and Conditions of Effective Speaking.
Experimental Investigations of Judgments of Effective Speech.
New Methods of Evoking Synaesthesia.
Comparison of Spoken and Written Style.

UNIVERSITY OF MANCHESTER. Department of Education.

Investigation of Partial Deafness in Children and Adults.
Correlation of Hearing for Pure Tones with Hearing for Speech.
Relation of Dumbness and Defects of Speech to Organic and to Functional Deafness.
Investigation into Methods of Educating the Partially Deaf.
Application of Quantitative Methods to the Study of Speech Defects.

NOTTINGHAM. University College.

Development of Language in Young Children.
A Psychological Study of Mathematical Theorems bearing on the Problem of the Nature of Intelligence.

UNIVERSITY OF READING.

An Enquiry into the Relations of Arithmetic and Geometric Abilities.

UNIVERSITY OF SHEFFIELD.

Children's Vocabularies.
Correlation and Mental Factors.
Analysis of the Learning Curve.

UNIVERSITY OF SOUTH WALES. Training Department for Men, Cardiff.

The Teaching of Welsh to English-Speaking Children.
An Investigation into Problems relating to Admission to Secondary Schools by Competitive Examination including use of Intelligence Tests.

UNIVERSITY OF SOUTH WALES. Training Department for Women, Cardiff.

Adolescence.
Literary Appreciation.
Study of the Characteristics of Infancy.
Critical Consideration of Performance Tests.

STRAWBERRY HILL, MIDDLESEX. St Mary's College.

Complete Vocabulary of Boy of Four Years of Age.

SWANSEA. University College.

An Investigation of the Relation between Bilingualism and Intelligence.

TENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

FIRST ANNOUNCEMENT

1. In accordance with the resolution at the Ninth International Congress of Psychology in New Haven, U.S.A. 1929, the Tenth International Congress of Psychology will take place in Copenhagen, from Monday, August 22nd to Saturday, August 27th, 1932.

H.M. the King of Denmark has consented to undertake the protectorate.

2. Psychologists and men of kindred sciences can participate as active members of the Congress. In addition, other persons interested can be admitted to some of the meetings as passive members.

3. The fee for active members is 150 francs and for their ladies 60 francs. The fee for passive members is 60 francs.

4. The official languages at the Congress will be: English, French, German and Italian.

5. The Congress will be divided into sections, according to the number and character of the papers to be read. Besides, it is intended to arrange some common symposia with lecturers specially invited.

6. During the Congress social gatherings will be arranged for the active members and their ladies, and there will be given an opportunity of seeing Copenhagen under capable guidance.

7. Reservation of accommodation during the Congress has been undertaken by The American Express Company.

8. In connection with the Congress, tours to Psychological Institutions in neighbour countries have been planned. The Committee of the Congress has been informed that Prof. Katz, Rostock, from the "Deutsche Gesellschaft für Psychologie" will be in charge with tours to German and Austrian Institutes. Prof. Katz will be pleased to supply any information on application.

For the National Committee
EDGAR RUBIN.

Visits to German and Austrian Institutes of Psychology following the International Congress of Psychology at Copenhagen.

Im Einverständnis mit den deutschen und österreichischen Mitgliedern des Vorstandes der internationalen Gesellschaft für Psychologie will der Vorstand der deutschen Gesellschaft für Psychologie den ausländischen, insbesondere den überseeischen Teilnehmern des Kongresses im Anschluss an die Tagung in Kopenhagen einen Besuch der grösseren deutschen und österreichischen psychologischen Institute ermöglichen. Mit der Organisation der Rundreisen ist Herr Prof. Dr D. Katz, Rostock, betraut worden. Herr Prof. Katz wird sich alsbald mit den in Frage kommenden Stellen in Verbindung setzen, um den Wünschen aller an diesen Rundreisen interessierten Kongressteilnehmern so weit als möglich Rechnung zu tragen. Es werden dann zu gegebener Zeit weitere Mitteilungen über die geplanten Rundreisen erfolgen. Etwaige Anfragen sind an Herrn Prof. Katz, Rostock, zu richten.

Following is an advance programme, the final form of which will be presented during the International Congress of Psychology in Copenhagen:

Leave Copenhagen for Rostock (Germany) on August 28th, arriving at Rostock on the same day. Inspection of the Rostock Institute on the 29th, leaving for Hamburg during the afternoon, and arriving there in the evening (of the 29th). Remain in Hamburg on the 30th and 31st, visiting the Hamburg Institute on the 30th. Journey to Berlin on the 31st, arriving in the evening of the same day. Remain in Berlin on the 1st, 2nd, 3rd and 4th of September, inspecting the Berlin Psychological Institute on the 1st, the Institute for Industrial Psychotechnics on the 2nd, leave for Leipzig on the 4th. Visit the Leipzig Institute on September 5th. On September 6th travel from Leipzig to Halle, Jena and Weimar, visiting the Institutes of Halle and Jena on September 9th.

Two Groups (*Group* 1 and *Group* 2) will be formed to continue the trip from Leipzig.

Group 1 leaves Weimar, or Leipzig, for Dresden on September 7th, arriving at Dresden in the evening of the 7th. Remain in Dresden on the 8th and 9th. Travel from Dresden to Vienna on September 9th, visiting the Vienna Institute on the 10th. Vienna marks the end of the trip. It is advised to return from this point through Innsbruck and Munich.

Group 2 leaves Weimar or Leipzig on September 7th for Göttingen, visiting the Göttingen Institute during the forenoon of September 8th, leaving in the afternoon for Marburg, arriving in the evening of the same day. Inspection of the Marburg Institute on September 9th, leaving for Frankfurt in the afternoon of the 9th, arriving in the evening of the same day. Inspection of the Frankfurt Institute on September 10th. On September 11th and 12th, travel from Frankfurt to Giessen, or Mannheim, or Heidelberg. Inspection of the Institute of Giessen or Mannheim on

September 11th. Travel from Frankfurt or Mannheim or Heidelberg to Würzburg on September 12th. Inspection of the Würzburg Institute on September 13th. Trip to Munich on September 13th or 14th. Visit the Munich Institute on September 14th. The trip of Group 2 ends at Munich.

Application by those who intend taking part in one or other of these tours of inspection should be made as early as possible to:

Prof. Dr D. Katz,
Rostock (Meckl.),
Moltkestr. 13,
GERMANY.

AN APPEAL

The Editor,
British Journal of Psychology,
c/o Cambridge University Press,
Fetter Lane,
London, E.C. 4.

Dear Sir,

A disastrous fire, during the night before Christmas Eve, destroyed a large portion of the Library of the University of the Witwatersrand, including many philosophical and psychological books.

The contents of the Library were only partially covered by insurance; and the prevailing depression makes it difficult to find the money for promptly replacing the losses.

May I, in these circumstances, appeal through the pages of the *British Journal of Psychology* to all fellow Philosophers and Psychologists, and especially to my friends and colleagues at British Universities, to help me in building up again, as soon as possible, an adequate collection for my students?

Every gift of author's copies or of duplicates—addressed to the Librarian, University of the Witwatersrand, Johannesburg, South Africa—will be gratefully acknowledged, and the name of the donor, together with the occasion of the gift, will be recorded in the books themselves.

Yours faithfully,
R. F. ALFRED HOERNLÉ.
Head of the Department of Philosophy.

NOTICE

RE GRANTS IN AID OF PUBLICATION OF *MONOGRAPH SUPPLEMENTS*

The Council of the British Psychological Society is prepared to make a limited number of grants to assist in publication of suitable MSS. as *British Journal of Psychology Monograph Supplements*, on or before March 31st.

Individual grants will not exceed £50, and will be given subject to repayment out of receipts from sales.

Applicants for grants should submit MSS. to the Editor, Prof. James Drever, The University, Edinburgh, on or before September 1st.

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